Vol. 38, No. 2 March, 1931

PSYCHOLOGICAL REVIEW PUBLICATIONS

Psychological Review

CONTENTS

PUBLISHED BI-MONTHLY
FOR THE AMERICAN PSYCHOLOGICAL ASSOCIATION
BY THE PSYCHOLOGICAL REVIEW COMPANY
PRINCE AND LEMON STS., LANCASTER, PA.
AND PRINCETON, N. J.

Entered as second-class matter July 13, 1897, at the post-office at Lancaster, Pa., under Act of Congress of March 3, 1879

PUBLICATIONS

OF THE

AMERICAN PSYCHOLOGICAL ASSOCIATION

EDITED BY

HOWARD C. WARREN, Princeton University (*Review*)
RAYMOND DODGE, Yale University (*Monographs*)
EDWARD S. ROBINSON, Yale University (*Bulletin*)
S. W. FERNBERGER, University of Pennsylvania (*J. of Exp. Psych.*)
WALTER S. HUNTER, Clark University (*Index and Abstracts*)
HENRY T. MOORE, Skidmore College (*J. Abn. and Soc. Psychol.*)
HERBERT S. LANGFELD, Princeton University, Business Editor

WITH THE CO-OPERATION OF

Many Distinguished Psychologists

PSYCHOLOGICAL REVIEW

contains original contributions only, appears bi-monthly, January, March, May, July, September, and November, the six numbers comprising a volume of about 540 pages.

PSYCHOLOGICAL BULLETIN

contains critical reviews of books and articles, psychological news and notes, university notices, and announcements, appears monthly (10 numbers), the annual volume comprising about 720 pages. Special issues of the Bulletin consist of general reviews of recent work in some department of psychology.

JOURNAL OF EXPERIMENTAL PSYCHOLOGY

contains original contributions of an experimental character, appears bi-monthly, February, April, June, August, October, and December, the six numbers comprising a volume of about 500 pages.

PSYCHOLOGICAL INDEX

is a compendious bibliography of books, monographs, and articles upon psychological and cognate topics that have appeared during the year. The Index is issued annually in June, and may be subscribed for in connection with the periodicals above, or purchased separately.

PSYCHOLOGICAL ABSTRACTS

appears monthly, the twelve numbers and an index supplement making a volume of about 700 pages. The journal is devoted to the publication of non-critical abstracts of the world's literature in psychology and closely related subjects.

PSYCHOLOGICAL MONOGRAPHS

consist of longer researches or treatises or collections of laboratory studies which it is important to publish promptly and as units. The price of single numbers varies according to their size. The Monographs appear at irregular intervals and are gathered into volumes of about 500 pages.

JOURNAL OF ABNORMAL AND SOCIAL PSYCHOLOGY

appears quarterly, April, July, October, January, the four numbers comprising a volume of 448 pages. The journal contains original contributions in the field of abnormal and social psychology, reviews, notes and news.

ANNUAL SUBSCRIPTION RATES

Review: $5.50 (Foreign, $5.75). **Index:** $4.00 per volume.
Journal: $6.00 (Foreign, $6.25). **Monographs:** $6.00 per volume (Foreign, $6.30).
Bulletin: $6.00 (Foreign, $6.25). **Abstracts:** $6.00 (Foreign, $6.25).
Abnormal and Social: $5.00 (Foreign, $5.25). Single copies $1.50.
Current numbers: **Review** or **Journal**, $1.00; **Abstracts**, 75c; **Bulletin**, 60c.

COMBINATION RATES

Review and Bulletin: $10.00 (Foreign, $10.50).
Review and J. Exp.: $10.00 (Foreign, $10.50).
Bulletin and J. Exp.: $11.00 (Foreign, $11.50).
Review, Bulletin, and J. Exp.: $15.00 (Foreign, $15.75).
Review, Bulletin, J. Exp., and Index: $18.00 (Foreign, $18.75).

Subscriptions, orders, and business communications should be sent to the

PSYCHOLOGICAL REVIEW COMPANY
PRINCETON, N. J.

VOL. 38, No. 2 March, 1931

THE PSYCHOLOGICAL REVIEW

A RESPONSE INTERPRETATION OF CONSCIOUSNESS [1]

BY HERBERT S. LANGFELD

Princeton University

The present century has given us a number of different movements or tendencies in psychology. This is a healthy sign in a new science, and the criticism that we do not know our own minds, even though literally and figuratively correct, need not disturb us. We are progressing in a crude trial-and-error fashion, but it is far better to wander and even to retrace our steps as far back as Aristotle if necessary, in order to pick up a right clue, rather than to remain contented in a blind alley such as was formed, for example, by a thorough-going and consistent doctrine of introspection. Nevertheless, I am not going to present a new theory. We in America have grown historically minded of recent years, after a few decades of air-mindedness, when we sometimes failed to realize that there was any firm ground beneath us, and we now know that it is dangerous to proclaim the newness of any theory. It is merely my purpose to present what seem to me important aspects of an old theory which is still in the making.

I

The view I desire to discuss has had several names, and has in recent years been known as the *motor theory*. This name was undoubtedly chosen because of the emphasis placed upon the efferent side of the nervous arc. But inasmuch as

[1] Address of the president before the American Psychological Association, Iowa City meeting, December, 1930.

there is already a tendency to over-emphasize this end of the response mechanism, and also because the term *motor* calls up a variety of meanings in the minds of those persons not already acquainted with the theory, it seems to me more desirable to substitute the word 'response' for 'motor.' [2]

Response means, as here used, response of the organism. In the response is included the entire afferent-efferent system, and the receptors, affectors, and central connecting paths. This definition distinguishes the sense of the term from that in which it is used by some behaviorists, who understand by it only the observable behavior of the organism, including the incipient responses. It is true that many behaviorists speak of a stimulus-response psychology, and at first sight it may seem a pedantic quibble to draw a distinction between the two phrases. But the term stimulus-response presents a very definite situation, stimulus at one end, response at the other, and a large gap between. The filling of this gap may belong in great part to the physiologists, but it cannot be neglected by psychologists to the extent to which some of the stimulus-response psychologists have been guilty of neglecting it. In fact it is exactly the importance of what goes on between stimulus and overt response that the response theory emphasizes. This feature of the stimulus-response psychology has been criticized by other than motor theorists. Bentley, for example, states: "I hold with those (and here the adherents of the doctrine of the *Gestalt* may be included with many others) who look upon the organism as yielding functional totalities, under the conjoint action of internal and external integrations and not as merely transmuting outside 'stimuli' into outside 'responses.'" [3] I believe it is safe to say that all except the extreme stimulus-response psychologists will agree with the above criticism. My object in discussing the point has been merely to correct a certain misunderstanding as to the position of the motor theorist.

[2] The term motor, however, will be used in this paper instead of response when, on account of the historical setting, it seems better able to convey the meaning intended.

[3] M. Bentley, Environment and context, Washburn Commemorative Volume, *Amer. J. Psychol.*, 1927, **39**, p. 59.

The outstanding difference between a consistent stimulus-response behaviorism and the response theory is in relation to consciousness. The former has no interest in consciousness. I do not say it denies it, because I do not know what it means to deny an unmistakable phenomenon, except if one does it as a matter of evasion. The latter's aim is in interpreting consciousness. Stimulus-response behaviorism, in fact, does not stand for a system in psychology, and I doubt if it cares to. It seldom holds to the subject matter of its study, as it defines it, and its epistemological errors have frequently been pointed out. It is a method of study, and as such has been of invaluable service to the development of psychology. The response theory, on the other hand, includes not only a method of study but an interpretation of mind, and is, in addition, aware of epistemological difficulties. It is in sympathy with that form of behaviorism which has been described by Smith and Guthrie, who write: "A behavioristic description of man's mind in no way contradicts the common sense assumption that men are conscious. We shall first find out what man *does*, and under what circumstances he does it, because this is open to observation and may be stated exactly. An understanding of behavior is essential to an understanding of consciousness." [4]

There has been a tendency in recent years for experimental psychologists to avoid a discussion of the nature and place of consciousness, the presumable reason being that the problem is one for the metaphysicians. This tendency is clearly seen in several of the best textbooks of the present day, where the traditional introductory chapter on the philosophical presuppositions has been omitted. Nevertheless, it is impossible to neglect the problem. Indeed it is not altogether ignored, even by those persons who believe that they do so, for their manner of thinking out their problem is inevitably colored by assumptions which they either consciously or subconsciously hold, and if the assumptions are too deeply imbedded in the subconscious, they are led into contradictions which suggest a certain naïveté. It is only fair to add that this

[4] Smith and Guthrie, General psychology, New York, D. Appleton & Co., 1921, 1 *ff.*

state of affairs is not peculiar to psychology. The assumptions underlying all scientific thinking have recently been admirably described by Emile Meyerson.[5]

The response psychologist frankly accepts the identity theory of the relation of consciousness to physiological processes. The two are one and the same. It is therefore entirely consistent to describe the subject matter of psychological investigation either in mental or in physiological terms. When describing the nature of psychological processes, consciousness may be considered an epiphenomenon. When describing mental states, the physiological is epiphenomenon. To speak of a process as epiphenomenon does not mean to banish it from one's world as useless. A description of mind in terms of mental phenomena helps physiological investigations and vice versa. Bayliss seems to have adopted this attitude toward the mental. According to Fearing, his view is that "The conditioned reflex technique is welcomed as a method of investigation of the functions of the higher centers, without an 'appeal to consciousness.' However, consciousness is not to be eliminated, but used 'as an indicator only.'"[6] Dualism and interaction are thus ruled out, and with them all semblance of vitalism. The response psychologist is strictly a monist and determinist, and in this respect is in accord with the great majority of experimental psychologists.

Such a concept of consciousness is as old as any of the other theories, but it has had more opposition, probably because of the materialistic trend which it suggests, its strongest advocate being La Mettrie, a professing materialist. It has even been intimated that Descartes, the father of a rigid dualism, really believed in such a monistic treatment of mind, but in order to remain conscious himself, was compelled pragmatically to place a guiding spirit in the head. Certainly most of his scientific observations smack of physiological determinism. Many a modern scientist follows the same queer turn after a most lucid description of actual phenomena. Curiously enough, physiologists seem particu-

[5] E. Meyerson, De l'explication dans les sciences, Paris, Payot, 1927.

[6] F. Fearing, Reflex action, Baltimore, Williams & Wilkins, 1930, p. 295.

larly fearful of the specter of philosophical materialism. Curt Goldstein, for example, who has contributed invaluable material for the psychologist, almost apologetically states that nothing is further from his thoughts than to try to explain mental phenomena through material functions. He only wishes to show the 'similarity between the nature of the bodily material functions and the mental phenomena.' [7]

Where the response psychology differs from the traditional monistic view of consciousness is in its identification of consciousness with the entire response, both afferent and efferent, and in its insistence upon the probability that consciousness does not occur unless there is a discharge into efferent paths and thence into the muscles. In other words, this identification with the complete arc means that both afferent and efferent discharges are necessary factors correlated with consciousness, and that a change in either path may produce a change in the conscious content. One hesitates to read too much into the words of writers who have lived in an age of science so different from our own, but La Mettrie's remark that ". . . since all the faculties of the soul depend to such a degree on the proper organisation of the brain and of the whole body that apparently they are but this organization itself. . . ." [8] seems on the face of it to imply such a relationship, and incidentally to show that, though a materialist, he did not go so far as the present day behaviorist in denying the phenomenon of mind. And to jump to the present era, G. H. Lewes has indicated a correspondence between consciousness and the activity of the organism as a whole, although he did not clearly emphasize the motor processes.[9] Dewey, in his article on 'The reflex arc concept in psychology,' [10] seems through his criticism,

[7] C. Goldstein, Das Symptom, seine Enstehung und Bedeutung für unsere Auffassung vom Bau und von der Funktion des Nervensystems, *Arch. f. Psychiat. u. Nervenkr.*, 1926, 76, 107.

[8] La Mettrie, Man a machine, Chicago, Open Court Publ. Co., 1912, p. 128. Quoted by Fearing, *op. cit.*, p. 112.

[9] G. H. Lewes, Problems of life and mind, Boston, Houghton, Osgood & Co., 1880.

[10] J. Dewey, The reflex arc concept in psychology, PSYCHOL. REV., 1896, 3, 357-370.

which was directed primarily against an analysis of the reflex arc into stimulus and response, to have suggested such a relationship and Dashiell has recently published a clear description of this view.[11]

Now the direct relation of the afferent path with consciousness seems almost obvious, while a similar relation of the efferent path appears to many not only not obvious but highly improbable, if not absurd. And it is for that reason, I take it, that the response psychologist or motor theorist has found it necessary to direct most of his attention to the gathering of such data as would throw light on the role which the efferent side plays in consciousness. It is this position that Holt has taken in his treatise on the function of specific response in cognition,[12] and Washburn in her theory of motor response and imagery.[13] It is to these two investigators that the theory owes most of its vitality. Indeed, it might well be said to have started with them, although Bain stated that "thinking is restrained speaking and acting," [14] and scattered references to the importance of motor function in relation to consciousness may be found throughout the literature. In fact, a number of psychologists who cannot be classed as response psychologists have undoubtedly added to its development. Münsterberg, for example, was very close to it in his action theory. As will be remembered, vividness was correlated by him with the openness of the efferent paths, which means that the less inhibition there is in the response to an object, the more clearly are we aware of the object. He also stressed the motor side of the affective processes, when he correlated pleasantness and unpleasantness with the extension and flexion of the muscles. Much as he emphasized the importance of the motor process theoretically, however, he was inclined to neglect it in his more concrete thinking. James, though a dualist, leaned toward a motor theory, as is

[11] J. F. Dashiell, A physiological-behavioristic description of thinking, PSYCHOL. REV., 1925, 32, 54–73.

[12] E. B. Holt, The Freudian wish, New York, Henry Holt, 1915.

[13] M. Washburn, Movement and mental imagery, Boston, Houghton Mifflin, 1916.

[14] Quoted by Dashiell, Fundamentals of objective psychology, Boston, Houghton Mifflin, 1928, p. 538.

plainly indicated in his ideo-motor theory and suggested at times in his theory of the emotions. There is also a passage in William Stern's writings which can be readily interpreted as emphasizing the motor factor in consciousness. He states: "The expression 'master of space' signifies that not only the development of space perception and imagery is to be discussed, but the conquest of the spatial in cognition and in action at the same time. The two are inseparable; the unity of the sensory and motor comes out nowhere so clearly as in connection with space; there is no comprehension without accommodation, no understanding without undertaking, no perception without a self-adjustment."[15] Most of the geneticists in the space perception controversy, about the time of Lotze and later, at least admitted the influence of the motor side when it came to the perception of the third dimension. Finally, attention might be called to the reference to motor processes involved in many of our words denoting mental functions, such as comprehension, apprehension, and perception itself.[16] Those who are against a motor theory may claim that we have here mere figures of speech. But figures of speech are often very suggestive for psychologists, especially in instances such as these where they refer to mental processes.

One of the consequences of the identification of consciousness with the efferent as well as the afferent nervous process is that such a theory runs counter to the most widely accepted view of the localization of the mind in the brain, a view which has been held with few exceptions since the early days of philosophical speculation, and became firmly entrenched in a form similar to that held today, when the sensory were distinguished from the motor nerves at the beginning of the Christian era. Many facts of physiology have seemed to strengthen the belief, notably the doctrine of phrenology and the more scientific revision into a strict localization of functions. The specific energy theory of Müller, though not

[15] W. Stern, Psychologie der frühen Kindheit, Leipzig, Quelle & Meyer, 5th ed., 1918, p. 85.

[16] See R. C. Givler, The intellectual significance of the grasping reflex, *J. Phil.*, 1921, **18**, 617–628.

accepted for long, undoubtedly added support to the belief, as did also the energetic and interesting arguments as to whether the cord, as well as the brain, was conscious. In the last century, this latter question took on great importance; each side had its strong supporters among the prominent physiologists of the time, and the controversy reached its height with Pflüger's attack on Lotze.[17] On the other hand, the results of recent experiments in the localization of functions, notably the research of Franz and of Lashley, which seem to indicate equal potentiality of brain function rather than exact localization, should have a tendency to weaken the hold which consciousness has on the brain as a place of residence, for along with the notion of exact localization should go the last remnant of a faculty psychology and centrally located perceptions. Another recent physiological concept which has been widely accepted and made much of, namely the function of the organism as a whole, should have the same tendency, for the theory certainly means that no part is independent of the other parts of the organism. Therefore it would seem to be illogical to correlate only the processes of the brain with consciousness.

Many experimental psychologists and physiologists may consider the problem of the seat of consciousness a merely academic one, but most of us show by our reasoning that we have a decided opinion in regard to it. Certainly one of the most widely accepted views of sensation and perception implies a central localization, the view, namely, that there is a stimulation of end organs, afferent current, sensation, efferent current, and motor effect. The very name, sensory nerve, as contrasted with motor nerve, is an indication of how firmly established this notion is. The motor theorist, to be consistent, should use the terms afferent and efferent instead of sensory and motor, even though they have the disadvantage, through the similarity of sound, of being easily confused with each other. A typical example of such a view is that of Woodworth's, which states that "sensation means the activity of the receiving organ (or sense organ),

[17] See F. Fearing, Reflex action, 1930, especially chapter 11.

of the sensory nerve, and of certain parts of the brain, called the sensory areas. Without the brain response, there is apparently no conscious sensation, so that the activity of the sense organ and sensory nerve is preliminary to the sensation proper." [18] Titchener, on the other hand, expressed himself most emphatically to the effect that mind is not correlated alone with the brain, or resident in the brain. He wrote: "There is no organ of mind; that phrase is an echo of the mannikin-mind. . . . The scientific fact is that, whenever we come upon mental phenomena, then we also find a functional nervous system. . . . *Size of brain and complexity of the nervous system are matched by range and complexity of mental phenomena.*" [19] This statement seems to indicate a correlation between consciousness and the entire nervous system in a manner similar to the view I have accepted. But Titchener rejected a motor theory, and inasmuch as he did not make the efferent paths a necessary factor for consciousness, he would probably have to agree in general with the statement just quoted from Woodworth. Knight Dunlap has recently expressed the opinion that more emphasis than in the past should be placed on the periphery and less on the brain.[20]

The physiologists have had a strong tendency to localize consciousness in the brain, and in general have been less critical of the assumption than have been the psychologists, to whom in great part they owe the notion (probably because the problem is not so vital to them). To select the physiologists of the past, Cabanis, one of the founders of physiological psychology, wrote of the brain that it is "a special organ whose particular function it is to produce thought just as the stomach and intestines have the special function of carrying on the work of digestion, etc." [21] And this conception seems to be the popular one in its essential features today.

[18] R. S. Woodworth, Psychology, New York, Henry Holt, 1929, p. 312.

[19] E. B. Titchener, A beginner's psychology, New York, Macmillan, 1915, 10–11.

[20] K. Dunlap, Response psychology, Psychologies of 1930, Worcester, Clark University Press, 1930, 313 *ff.* In his article, Psychological hypotheses concerning the functions of the brain, *Scient. Mo.*, 1930, 31, 97–112, he has more fully described his view of the relation of consciousness to the brain, which is similar to the one I have just outlined. Unfortunately I did not see his paper until after I delivered my address.

[21] Quoted by Fearing, *op. cit.*, p. 89.

Sherrington in his directions for decapitating a cat ends with the considerate remark: "The head thus severed is pithed and thrown away." [22] He was at least not taking any chances. Herrick's description of cortical equilibrium seems evidently to have been influenced by the idea of the central seat of consciousness. He states: "When the equilibrium of the resting cortex is thus disturbed and nervous excitations irradiate from the focus of the initial excitation, the process will continue until a new equilibrium is established—it may be by motor discharge, it may be by fatigue with no practical outcome, it may be by the fabrication of a new pattern of cortical activity or a new enduring 'set' of the reacting system which modifies all subsequent activity of this system and may appear in consciousness as an idea, a judgment, a decision, a purpose, or an ideal." [23] Examples could be taken from many more of the leading physiologists.

Now an advantage in the response point of view, apart from the light which the emphasis upon the efferent processes, together with an examination of their function, may throw on the fundamental problem of the nature of consciousness, lies in the fact that it enables one to maintain a strictly deterministic, monistic, and non-vitalistic interpretation of human activity. Not that an opponent of the motor theory is necessarily a vitalist, or even a dualist, but there is always a possibility of such a slant occurring in his descriptions and explanations, especially if in his opinion consciousness is definitely localized in the brain. Examples may be taken from several of the authors just quoted. Herrick remarks: "Consciousness, then, is a factor in behavior, a real cause of human conduct. . . " and further ". . . if consciousness, when present, is a real factor in the causative complex resulting in behavior as I believe it to be, obviously this factor cannot be ignored in a scientific analysis of the field of behavior as a whole." [24] And the same attitude seems by

[22] C. S. Sherrington, Mammalian physiology: a course of practical exercises, London, Humphrey Milford, 1929, p. 193.

[23] C. J. Herrick, Neurological foundations of animal behavior, New York, Henry Holt, 1924, 246–247.

[24] C. J. Herrick, *op. cit.*, 304–305.

inference to have been held by Sherrington when he wrote: "To these ancient and variable reflexes, consciousness, in the ordinary meaning of the term, is not adjunct. The subject as an active agent does not direct them and cannot introspect."[25] Finally, attention might be called to Cannon's attack upon the James-Lange theory of emotions, which in turn has been severely criticized by several of the leading psychologists. It is undoubtedly agreed that his empirical findings have been of inestimable value in the furtherance of our knowledge of the nature of the emotions, but the importance of his theoretical deductions has been lessened, principally, it seems to me, through his bias for a central localization of consciousness. This bias appears very plainly where he insists upon calling the reactions of a cat without visceral connections true emotions, and those of a decerebrated cat with visceral connections intact sham emotions.[26]

II

So much for the theoretical discussion. And now for a consideration of some of the empirical facts in support of the theory. One of the most persistent criticisms of a response theory is, to put it very simply, that motor response remains motor response and that perception, if it exists at all, has for its content either movement of muscles or muscle systems, or merely kinesthesis. This conception must have been in the minds of the critics of a genetic theory of space perception who maintained that it was absurd to believe that the perception of muscular movement could ever be like the perception of space.

I have attempted in a previous paper to correct such misunderstandings of the response theory.[27] Of course the perception of kinesthesis is not the perception, for example, of form or of movement such as is experienced in the phi-

[25] C. S. Sherrington, The integrative action of the nervous system, New Haven, Yale University Press, 1906, 387–388.

[26] W. B. Cannon, The James-Lange theory of emotions: a critical examination and an alternative theory, Washburn Commemorative Volume, *Amer. J. Psychol.*, 1927, 39, 106–124.

[27] H. S. Langfeld, Consciousness and motor response, PSYCHOL. REV., 1927, 34, 1–9.

phenomenon. As soon as there is a perception of kinesthesis, there is not a perception of movement. This fact, which is a fundamental one in a motor theory, may perhaps best be illustrated by an example from empathy. It is agreed, I believe, even by persons who are in general opposed to a motor theory, that there are motor responses at times to objects, and that these responses appear to be projected into the object. (This is not the concept of empathy as originally propounded by Lipps. He did not admit the motor response but spoke in terms of the projection of ideas. It is, however, the concept which is generally accepted, at least by experimental psychologists today.) Even Titchener was in favor of such a theory. It is supposed, however, by many persons that these responses are caused by the perception of the object, or merely follow it, and thus become an associated sequence. What actually happens, so far as I can observe, when we have true empathy, is that there is an immediate perception of some characteristic of the object, such as grace of line, or abruptness of curve, but no perception of kinesthesis at the time of this perception of grace or of abruptness. The perception of kinesthesis occurs only upon introspection of one's reactions, and with such introspection empathy ceases, and with it, in turn, the grace of the object so far as the observer is concerned. As a further example, let us examine the perception of the weight of an object in the lifted weight experiment. For the ordinary observer, the object appears *heavy*. For the introspectionist, it is a matter of kinesthesis. In the former case there is empathy, and in the latter, not. For the naïve observer, the idea of muscular response does not ordinarily occur in consciousness. Nevertheless, according to the response theory, at least incipient motor responses do occur, and the efferent impulse in connection with these is a physiological factor underlying that peculiar quality of the object called weight, and without such responses, there would be no perception of weight. (One might contend that the kinesthesis as sensation was subconsciously present, and entered into a higher mental synthesis, but I do not think that there is any justification for

such an assumption.) Also, without an efferent response, following proprioceptive stimulation, there would be no kinesthesis. For according to the response theory, the afferent proprioceptive excitation does not become a perception until there is a motor response of some kind. In short, the judgment of the weight of an object and the judgment of kinesthesis involve, to a certain extent, different motor responses. The physiological conditions are different in the two cases, and so are the perceptions. It follows logically from this response concept of perception that the introspective method, in the narrower sense, does not give us an adequate account of mental phenomena, since the response theory supports the well-known contention of those opposed to a thorough-going method of introspection that a shift of attitude changes the content under observation.

Washburn is presumably in accord with the view of perception just outlined, for she states: "Now the reactions themselves, of whatever nature, give rise to kinesthetic excitations, and this whether there are movements fully performed or incipient, tentative movements only. These kinesthetic excitations are a regular accompaniment of such excitations from outside stimuli as are reacted to at all; they influence all conscious processes. Whether they themselves are directly represented in consciousness depends on whether they in their turn are reacted to." [28] The term 'kinesthetic stimulation' was the only point of possible disagreement, but I have learned from Professor Washburn that 'proprioceptive' could be used in this context for 'kinesthetic.'

In order still further to illustrate the role of response in perception which has just been outlined, one might take for example the process of learning to write on the typewriter. In the early stages of learning, there is a frequent shift in the conscious content from the visual perception of the words to be copied to the feel of kinesthesis from the finger movements. In this stage the latter content predominates. When the process becomes well learned, kinesthesis drops out of consciousness, the proprioceptive stimulation becomes merely

[28] M. Washburn, *Gestalt* psychology and motor psychology, *Amer. J. Psychol.*, 1926, 37, p. 519.

the dominant factor in the smooth coördination of the motor processes, and the visual perception acts as a cue for this process, which has become conditioned to the visual perception. It should not be assumed, however, that the finger movements are the essential efferent responses underlying the visual perception (although they may perhaps alter to some extent that perception), for obviously the visual cue must have been a visual perception before the occurrence of the finger responses in order to act as a cue at all, and consequently must already have had its own specific response, perhaps in terms of vocal-motor activity or eye-movements. This difference between the specific response to the visual stimulus and the response in terms of finger movements is readily seen in the phenomenon of overlapping in typewriting, namely, the fact that the finger movements often lag several words behind the word which the operator is reading. Another example of such a shift may be taken from a game involving skilled movements, such as golf. When the swing of the drive is 'working well,' one is clearly conscious of the ball to be struck and the ground beneath. When there is imperfect coördination, one 'feels one's muscles,' that is to say, one reverts to the learning stage and the visual impressions become secondary. To be set for kinesthesis during the drive is fatal for a good shot. A systematic introspectionist on the links would be a poor golfer, and, one might add, a poor psychologist as well.

A somewhat analogous situation arises in regard to the problem of meaning, which problem has been met in such a satisfactory way by several response psychologists, notably by Holt with his theory of specific response, that it has even gained the approval of psychologists such as Boring, who have in other respects been very conservative in their estimation of the value of a motor interpretation of consciousness.[29] The example I refer to is in connection with reading. Here again the cues are visual stimuli which have their own underlying responses whenever there is a perception of such

[29] E. G. Boring, A history of experimental psychology, New York, Century Co., 1929, p. 588.

objective stimuli, and the meaning is carried by other responses which are conditioned to the same visual stimuli. I venture to suggest, however, that when one is engrossed in reading, that is, when one is set entirely for the meaning of the visual stimuli before one, and has no difficulties from the visual side, one is very little conscious of the words as printed, and at times even totally unconscious of them. This state of mind is described by Sherrington in regard to writing as follows: "As I write, my mind is not preoccupied with how my fingers form the letters; my attention is fixed simply on the thought the words express, but there was a time when the formation of the letters, as each one was written, would have occupied my whole attention." [30] The relatively loose connection between the specific responses from the visual cues, and those underlying the meaning of one's thought, is well illustrated by the fact that one can very readily read out loud and think of something else entirely different. In this problem of meaning there is involved the problem of imagery, but explanation of imagery in terms of incipient response is too well known to need any description in this place.

To return to visual perception, a number of instances may be cited which tend to show the influence of motor response on the visual phenomena. One of the most frequently used examples is that of eye-movements as an explanation of optical illusion, including that of apparent movement. The most serious of several criticisms against the use of such an explanation is that the phenomena would often call for movements which, on account of their antagonism, would be impossible for the eye to carry out. Inasmuch as the response theory does not call for actual movement, but merely a specific change in the efferent system, it is possible that a change in muscle tonus produced by the antagonism of the muscles involved is a physiological cause of such phenomena. Further, although the theory that eye-movements underlie visual phenomena seems a most logical one,

[30] C. S. Sherrington, The integrative action of the nervous system, 389–390. Quoted by Fearing, *op. cit.*, p. 292.

and one which appears to apply in many instances, I have not felt that the movements need be restricted to the eye-muscles, but rather that any other response might, under certain circumstances, be conditioned to the visual stimulus.[31]

Then there is the effect of compensatory movements under certain conditions of rotation. If one is rotated with the head bent at an angle of ninety degrees to the shoulders, and then directly after rotation the head is straightened again, the subject often sees the floor sloping away from him, and it seems to him that he is about to slide down the incline. The compensatory movements of arms and legs fit into this situation. Now the visual stimuli have not altered, but the stimuli from the semi-circular canals have changed the muscle tonus, and these changes, it would seem, underlie the alteration in the visual perception. Further, we have the well-known phenomenon of the changed appearance of the landscape when looked at upside down. Until readjustments are made—and these readjustments would certainly seem to be motor in character, for they are readjustments of orientation—the landscape appears entirely unfamiliar. And while on the subject of change in the perception of the environment, attention might be called to the pathological condition, generally referred to as estrangement of the world of reality, where the patient feels that life is very like a dream. The description by soldiers in the late war of their experiences shows evidence of a similar condition. They speak of the seeming unreality of what is going on about them, as if they were witnessing a play. There is a strong probability that a change in motor responses is the primary factor in this feeling of unreality, and the change in emotional responses is secondary to the motor changes.

Gesell has given us a very interesting picture of the genesis of visual form during infancy, which seems to me to point strikingly to the importance of the motor function in this regard. He took moving pictures of an infant's per-

[31] Washburn has discussed the application of a motor theory in explanation of illusions in her recent paper, A system of motor psychology, Psychologies of 1930, Worcester, Clark University Press, 1930, p. 88.

formance with a three-hole form board at eleven advancing age levels from twenty weeks to two years. The spontaneous behavior was observed and photographed at lunar month intervals. Each observation period lasted a few moments. At first there was evidence of a great deal of random movement with the round disk that fitted the round hole. During the same period, the infant's hand went over the contour of the disk and the edge of the round hole in the board. This exploration with the hand went on for a considerable period of time with little, if any, evidence of the infant's realization of the relation of disk to hole. Finally, however, at the age of one year, the infant took the disk and placed it in the hole as if the problem were a very simple one for it to perform. Now according to our theory of the perceptual process, the visual stimuli and afferent processes were not sufficient to give the infant the visual perception of form, until the motor processes had become organized in relation to the circle. Then the circles, in the two instances of disk and hole, were perceived. The subsequent act, that of placing disk in hole, followed upon this perception, perhaps by so-called insight. This idea of organization in the motor field will probably be accepted by the Gestalt school, for Köhler explicitly states that he does "not deny that the problem of organization exists in the field of movements and kinesthetic experiences." [32] He probably would not agree, however, that motor organization was first necessary before the visual perception could occur.

The association of ideas obviously finds its best physiological explanation for the motor theorist in the principle of Bok's reflex circle, and the conditioned response. In order thoroughly to understand the situation, it is also necessary to have an adequate knowledge of such processes as inhibition and facilitation of nerve impulses. Space does not permit my describing the valuable contributions by Holt to an understanding of these neural mechanisms.[33] Nor can I do more than touch upon the ancient theory of association of

[32] W. Köhler, Gestalt psychology, New York, Horace Liveright, 1929, p. 168.

[33] E. B. Holt, Animal drive and the learning process, New York, Henry Holt, 1931.

ideas on the basis merely of contiguity. The Gestalt psychologists have quite rightly challenged the theory and, true to their own system, have insisted upon an organization in the sensory field. But after all, I doubt whether there are many psychologists today who hold to such a simple explanation as contiguity, for they must be acquainted with the experimental results on retention and therefore know that there is no learning without attention to the problem and intention to learn. It remains, however, to describe what one means here by such words as attention and intention. A brief explanation of the process of association would be that exploratory responses execute a pattern over the objects which are to figure in the association and this organic pattern is the total response to the situation. Washburn has described the process very well when she states: "Not the fact that the stimuli A and B occur together gives to A the power of calling up an image B, but the fact that the two are attended together. And whatever else attention may mean, the fact is reasonably certain that a simultaneous attention to two things means a simultaneous motor response to them. The dependence of association upon attention and the sensual motor phenomenon becomes comprehensible, if we think of association as being itself primitively an association of movements." [34]

In regard to the association of ideas, I should also like to add that no response psychologist with any knowledge of the recent findings in physiology could very well assume that the response system is composed primarily of fixed reflex arcs. Coghill has clearly shown that the nervous system, at least the central and motor components, begins in a certain sense as a network, and that more specific forms of response subsequently develop from this primary pattern,[35] and Curt Goldstein has made the following statement in regard to

[34] M. Washburn, The function of incipient motor processes, PSYCHOL. REV., 1914, 21, p. 377.

[35] See his recent article, The structural basis of the integration of behavior, *Proc. Nat. Acad. Sci.*, 1930, 16, No. 10, 637–643. Also C. J. Herrick's article in the same number, 643–650, Localization of function in the nervous system. Both authors emphasize the action of the organism as a whole.

diffuse conduction in response: "On the basis of histological structure we conclude that the nervous system is a network. This network offers an unified mechanism, a system in which every stimulus produces a change in the total system. This change is represented internally by the conscious processes, externally by the movements of the effectors." [36]

The narrowing of the action pattern through experience to a greater degree of specificity is well exemplified in the genesis of emotion. The work of the Shermans has shown how little differentiated the so-called emotional reactions of young infants really are during the first days. The differentiation which develops is chiefly brought about by the patterns of response which have been learned in the course of adaptation to environment. In the authors' own words: "Out of this matrix of undifferentiated behavior the clearly adaptive emotional responses develop. What we call the emotions, therefore, are not present in the infant at birth, but develop, as a result of experience, as specific patterns of response. The infant does show certain types of reaction, such as passivity, aggressiveness, and withdrawal, which later, as the child learns to differentiate between varying situations, and establishes different responses to them, we call emotions. But this development begins immediately after birth with the handling of the child, who eventually learns to respond with specific reactions because of the training he receives." [37] In the light of such facts regarding the development of emotions, facts which oppose the notion of innate modes of response and action patterns, it becomes understandable why facial expressions have been found to be so ambiguous when judged apart from the stimulus and the general behavior of the individual.

The higher thought processes, rather than perception, seem to have been the first to be described in response terms. Previous reference has been made to Bain's remark that thought is incipient speech response and action. Ribot

[36] C. Goldstein, *op. cit.*, 84–108.

[37] M. and I. C. Sherman, The process of human behavior, W. W. Norton & Co., 1929, p. 145.

wrote that "a thought is a word or an act in a nascent state—a commencement of muscular activity." [38] There is the well-known fact that Mach referred to reasoning as a thought experiment, and Dewey has clearly described the process of mental trial-and-error behavior. Both Mach and Dewey's expositions lend themselves readily to an explanation in terms of muscular activity. It is not here a question alone of subvocal thinking, but of learning to meet a new situation in terms of incipient rather than overt responses. Thinking is a great time saver. For the response psychologist, thinking is in all essentials exactly the same as the overt behavior of the individual, condensed into more economical responses. Insight, it seems to me, is a result of such thought experiment. Among the numerous examples of such a moment of insight in animal behavior, I have chosen one from the writings of Tolman: "The act of clawing at the loop, or of entering a given alley I would assert to be conscious in cases where the animal can be observed to hesitate for a moment between clawing and not clawing or between entering and not entering." [39] According to a trial and error response method of thinking, one would look for a correlation between degree of motor coördination and intelligence. Ordinary observation does not seem to find such a positive relation. We do not necessarily associate a high degree of intelligence with skill in athletics, yet it is interesting to note that an intimation of a correlation between intelligence and motor coördination was observed by the Shermans in their experiments on infants.

III

Two of the most fundamental problems of psychology remain unsolved, and so far as I can see, response psychology has no entirely satisfactory answer at present. They form the psychological no-man's land still haunted by the vitalistic ghost. The one is the question of the so-called secondary qualities. No characteristics sufficiently specific have been found, on either the afferent or the efferent side of the nervous

[38] Quoted by Dashiell, Fundamentals of objective psychology, Boston, Houghton Mifflin, 1928, p. 538.

[39] E. C. Tolman, A behavioristic theory of ideas, PSYCHOL. REV., 1926, 33, p. 367.

system, to account for them. The behaviorist's explanation, in terms of verbal response, does not seem adequate. Weiss has seen the difficulty of such an interpretation, for he admits that the verbal discrimination is an acquired reaction very different from the first responses of the child.[40] For anyone who believes in the consciousness of secondary qualities, the verbal response is a conditioned response to an experience whose existence must first be explained. And therefore verbal response as an answer merely begs the question. Washburn has rightly remarked that "blueness is neither in the stimulus nor in the response, for these are movements and blueness is not a movement." [41] It is to be hoped, nevertheless, that the physiological correlate of the consciousness of blue and the rest of the secondary qualities may be discovered. A thorough-going response psychologist would naturally hazard a guess that the efferent side of the response will be found to be an important part of the physiological picture.

The other unsolved problem is that of the determination of what final common path the afferent impulses are to take in the special cases of audition and vision. It is very easy to say that when one is given instructions to move the right hand when one sees a triangle and the left hand when one sees a square, the figures become associated with the movement, but it must be remembered that the visual stimulus can fall on almost any part of the retina and still cause the correct reaction. Certainly no experimental psychologist is going to suggest a mental traffic policeman to guide the impulse, but by what mechanism has the afferent impulse been able to reach the correct efferent path? This problem has been, I believe, the most important one in the minds of the Gestalt school, and they have made a very courageous attempt to solve it, but organization and dynamic relations must remain mere terms, suggestive for further investigation, until we know more about the processes in the nervous system.

[40] A. Weiss, A theoretical basis of human behavior, Columbus, R. G. Adams & Co., 1925, p. 198.

[41] M. Washburn, Introspection as an objective method, PSYCHOL. REV., 1922 29, p. 92.

It must be confessed that response psychology cannot in its present stage lend much help, except in respect to the fact that organization and dynamics should be looked for quite as much in the response as in the afferent system and so-called brain field.

I have tried in this hour to explain the principles of the response theory as I understand them, and to describe some of the facts which seem to support them. There are many more problems than the two just mentioned which are baffling to response psychologists, and many phenomena which at present seem contradictory to their theory; but as I intimated in the beginning, I have not meant to present a finished system of psychology. Perhaps the fact that it is not finished is its greatest virtue.

[MS. received January 5, 1931]

ERRORS IN RECENT CRITIQUES OF *GESTALT* PSYCHOLOGY
I. SOURCES OF CONFUSION

BY RAYMOND HOLDER WHEELER, F. THEODORE PERKINS, AND S. HOWARD BARTLEY

University of Kansas

Introduction.—Within the last five years there have been many attacks upon *Gestalt* psychology not one of which has failed to distort the fundamental concepts which make it a unique development in the history of the science. These criticisms, therefore, place *Gestalt* psychology in a false light. They confuse it indiscriminately with tenets explicitly denied by *Gestalt* contributors and miss the basic discrepancies between the two positions, the old and the new. The uninitiated reader would gather the impression that there are no new issues involved, and obviously this is the impression that certain of the criticisms are intended to make. It is the purpose of this series of papers to correct the misinterpretations and inadequate conceptions of *Gestalt* now so prevalent, by answering the several critiques that have recently appeared.

In general, these criticisms are premature, superficial and self-contradicitory. Such a situation, perhaps, is to be expected, for often in the history of a science, criticisms are launched against a new movement before it is completely understood and before the consequences of its logic are fully thought out by the originators themselves. As it is, however, the chief misunderstandings of *Gestalt* psychology are traceable to a variety of conditions and the nature of these conditions must be understood before the basic issues can be made clear. To save reiteration, the present article, therefore, will be devoted to those general considerations, and detailed comments on various specific errors will appear in subsequent discussions.

Meanwhile it should be remembered that we are presenting the logical framework of *Gestalt* psychology as a whole, amplifying its assumptions and perhaps broadening its principles. There are inconsistencies in the *Gestalt* literature, and it has changed much in the last five years. Perhaps the critiques answered in this series would not have contained all of their claims had they been attacks upon the exposition of the *Gestalt* logic here presented. But why resort to alibis when the critics have had the same chance to build constructively upon the foundation laid by Wertheimer, Koffka and Köhler as anyone else, instead of spending their time finding fault with it? We are assuming, finally, that any inconsistency or disagreement found among configurational writers themselves is not worth taking seriously unless they are reversions to atomism.

1. *Inadequate conception of the history of psychology.*— Unfortunately an adequate, *interpretative* history of psychology has never been written. It is not surprising then, that critics vary in what they find in *Gestalt* psychology—vary, in fact, in the extent to which they understand the system. It is unfortunate, also, that our foremost history of psychology, based upon the assumption that progress comes through unnoticeable increments,[1] should have emphasized this point at the expense of major events in history which, we shall attempt to demonstrate in a subsequent paper, are quite the reverse. Progress by spurts, declares Boring, is an illusion, and commits one to 'machinism' in the history of science.[2] Boring is thinking of the psychological masses and is thinking of the individual contributor in terms of an interpretation that would be put upon him by the masses. Even so, progress is far from a smooth continuum. There are revolutions in history quite as real and significant in their contributions as explosions in nature. Neither one violates the logic of continua any more than the occurrence of a cyclone violates the laws that pertain to a steady breeze.

[1] E. G. Boring, A history of experimental psychology, New York, Century Co., 1929, 158, 368.

[2] E. G. Boring, The *Gestalt* psychology and the *Gestalt* movement, *Amer. J. Psychol.*, 1930, 42, 309.

Even the reading of history is fraught with danger. Just as the most important and potent forces in the world around us must be inferred from measurements of restricted manifestations of energy here and there, so the most powerful and lasting of concepts and points of view in the history of thought must be inferred from the printed word. They exist in the form of implications and assumptions—general undifferentiated ideas, often inarticulate but more often inadequately expressed by the author himself. Not until a system is studied as a whole in light of trends that cover centuries of time, and not until the total logical framework giving meaning to its various parts is evident, can the real nature of the system be ascertained. The writers of history, in the field of psychology, may justly make claim to such an accomplishment, but as will become evident, the most potent of basic concepts that have conditioned the form of psychological systems through the ages, in their relation to *Gestalt* psychology, have been overlooked. It is too easy to miss the tree for the leaves. The proof of this assertion lies in the self-contradictory nature of recent criticisms leveled against the configurational movement.

Space does not permit the reviewing of history here in sufficient detail to bring out more than one main trend which, because it has been missed, results in so much confusion at the present time, but this is perhaps the most important one. Man has always attempted in some fashion to account for the events that he observed around him. Primitive man did what we all do when we are confronted with a situation too large for us to handle. Relative to his degree of intellectual development, he reduced the problem to its simplest terms. He could not comprehend the wholeness, the organized character of the world about him; he saw only certain parts which, for him, emerged as wholes upon an undifferentiated ground. And he saw only one part at a time. Likewise a young child, looking at a picture, will see only certain isolated, unrelated objects: a chair, a table, a woman, a cat. The picture has no explicit unity. It is not the complex object which the adult sees. Each object within the picture is a segregated whole, a discrete, preceptual entity.

For primitive man, then, a given event was an entity, a discrete happening, unrelated to anything else. It was a whole involving no parts, something homogeneous, simple, unanalyzable and unanalyzed. What then, caused this event? Since it was perceived as a segregated whole, the answer had to be, 'Something within it; a will; a spirit.' A spirit within the tree made the tree grow; a spirit in the sky made it thunder; a spirit in the avalanche made it tumble down the mountainside. And this spirit was another discrete, separate existence, an independent entity. *There was no concept of organization. The world was a pluralistic chaos.* There was a *deus ex machina* for every object and event. Awareness of organization had not emerged from responses that were themselves organized. Organization was explicitly unthought and unverbalized, as is presumably the case with animals. Causation, therefore, was the influence of one discrete entity or part upon another. Here was implicit atomism, machinism.

This primitive idea of wholeness and unity persists even to the present day. It prevailed in the philosophy of Aristotle and still prevails in theology. Unity was synonymous with simplicity and homogeneity of structure. Logical impossibility of analysis, indivisibility and indestructibility depended upon the same *one-ness* of content. Thus, the soul could think of only one thing at a time, otherwise its unity would be ruined; and even today the layman clings tenaciously to the thought that consciousness is something qualitatively simple and incapable of change, in an effort to preserve his belief in the integrity of his immortal soul. Permanence still means to the average person, homogeneity and simplicity of structure, phenomenological simplicity. *It was in this primitive notion of one-ness that the descriptive unit, the element or atom, destined to dominate thought for centuries, had its origin.* Each atom, although endowed with force and movement, was a thing unto itself; nothing existed between the atoms; nothing between the blocks of energy. The world was still a pluralistic chaos.

With the evolution of human thought, man became able

to envisage a larger situation somewhat as a whole, a whole that was complex. *Then the problem of unity became a problem of organization.* But again the problem was too comprehensive; and again it was reduced to its simplest terms. Man still observed only the parts and continued as his primitive ancestors had done, *to find causality between the parts.* He took the only course; *he built an explanation of the whole upon his knowledge of the parts.* His parts were indivisible wholes, simple and homogeneous; they were elements or atoms. When he found it necessary to fall back again upon a spirit that was now to play a more extensive rôle in causation, a world soul that accounted for law and order among the independent and unrelated atoms, that soul was structurally still homogeneous and simple—a glorified arch-atom ruling all the others. Thus, in Ancient Greek philosophy, man launched upon the second phase in the development of his concept of wholeness and constructed a whole that must be derived from its parts. This concept of unity has dominated scientific thought practically to the present day, and its development has always taken one of two directions, 'machinism' or vitalism. It need not be explained, here, that mechanistic thought is in no way the exclusive type of deterministic system, but it should be pointed out that mechanistic logic is of necessity atomistic; *deterministic logic is not.* All mechanistic systems involve reasoning from parts to wholes.

It was not long before it became evident that a whole derived from unchangeable elements or parts, was an artifact. Accordingly, as a substitute for the world soul, or any vitalistic power that must account for harmony and organization among fixed atoms—an aggregation of parts, a machine—there developed the notion of creative synthesis. The unity of the whole then came to be explained as the product of a uniting process. This may be considered as the so-called scientific concept of unity, for it played an important part in the history of every science. It is the sort of whole that has prevailed in psychology especially since the time of John Stuart Mill. It is the whole regarded erroneously by the

majority of contemporary psychologists as the sort of whole that the configurationists are talking about. *It has occurred to but few readers of Gestalt psychology, evidently, that the type of whole meant by a Gestalt is as different from a synthetic whole as the synthetic whole was different from the whole of primitive man.* Configurational psychology declares and has proved that the synthetic whole is as much of an artifact as the unchangeable element.

For *Gestalt* psychology, synthesis is just as unmanageable, just as inadequate a concept as 'world soul,' or any vital force separable from a machine but resident within it, the power that unifies it and makes it run. And yet, as we shall see, Wundt and Titchener have been declared configurationists, and the categorical assertions have been made over and over again that there is nothing new in *Gestalt* psychology. We have reverted to Aristotle! The relativistic movement in physics, the organismic movement in biology, and the configurational movement in psychology represent a repudiation, insistent and complete, of this second type of whole, the derived whole; they substitute for it an *entirely different descriptive unit. In the evolution of scientific thought, we are passing into a third phase which is just as unique in its logic, as compared with the science of the past, as the notion of a complex but derived whole was unique as compared with the whole of primitive man, whose unity accrued to an original simplicity and homogeneity.*

We have seen that, in the original type of whole, unity meant a permanent homogeneity and simplicity of original content. In the second type of whole unity meant a derived homogeneity and simplicity of content achieved when parts lost their identity upon fusing together. How this mysterious qualitative change came about we were never told for the perfectly good reason that the theory was an impossible one. Indeed, most of the advocates of synthesis conveniently avoid the issue by not seeing the problem. One minute the parts were observed; the next minute they were gone and a new whole was in evidence. The parts were not left lying around anywhere. *They* must have fused. Did a spirit do it?

Yes, synthesis did it, *creative* synthesis. The advocate of synthesis cannot legitimately defend his position by saying that he was not trying to explain, but was only trying to describe, for to describe something inferred, is logically an effort to explain. His difficulty lay in not detecting the larger whole in which the so-called 'synthesis' occurs. The 'union' of chemical elements, for example, is not synthesis, when fields of stress—thermal, electrical, mechanical (pressure), gravitational—are all necessary for the process. Seen in the light of fields of force, 'synthesis' is just another case of *differentiation* of energy patterns from relative homogeneity to relative heterogeneity—precisely what is going on in any growth or evolutionary process, not a case of getting wholes from parts, but a process of getting parts from wholes. The picture is one of an expanding, differentiating whole.

The third phase in the evolution of the concept of wholes is not, and never has claimed to be, a recognition of wholes for the first time, practically every critic to the contrary notwithstanding. The issue has always been one of wholes. That was what the original atomism and the world soul were all about; it was what association and attention psychology were all about; it is what *Gestalt* psychology is all about. The *issue* is not an emphasis upon wholes (yet Boring [3] implies this several times in his history of psychology), and Robinson [4] holds a 'little German band' up to ridicule for something it never did. The issue in *suddenly* changing from a synthetic whole to an organismic whole is: What to do with wholes? That wholes must be treated in their own right is also a claim of *Gestalt* psychology but not by confession or otherwise a new claim. *The distinctive contribution of Gestalt psychology lies in an interpretation of what that statement means.* It means that wholes must, as scientific data, be used as principles, not alone admitted as facts; it means that a whole of whatever degree of phenomenological complexity, relative to a given problem of explanation, *must be the unit, and is the only unit.* This unit is as large and

[3] Boring, *loc. cit.*, 156, 158, 368, 499.

[4] E. S. Robinson, A little German band, *New Republic*, 1929, 61, No. 782, 10–14.

complex as the event to be explained, and contributes nothing except under the laws of the whole. Causation never runs in any direction but from a whole to its parts; never from one whole to another whole, that is, never from part to part. *Part to part situations yield indeterminism.*

Wholes are both logically and genetically primary. Unity is synonymous with organization, and phenomenological complexity is subordinate to functional unity. Functionally, complexity is constant; the atom is as complex as the universe; as many principles are required to account for the simplest as for the most complex thing in existence. A steady breeze obeys as many laws of dynamics as a cyclone; a drop of water is as hard to explain as a snow crystal; the behavior of man is no more complex in principle than the fall of an apple. Degrees of complexity, or of differentiation of energy-fields, belong on the side of phenomenology—structure—but forms or configurations of whatever degree of complexity are equally simple in functional plan. In terms of dynamics, there is no mind *versus* matter, man *versus* beast, organic *versus* inorganic, plan *versus* chaos, complexity *versus* simplicity, mechanism *versus* teleology, life *versus* inertness, blindness *versus* purpose. The same principles that account for an air current account for purposeful behavior, the same laws of dynamics that make gravitational systems intelligible are the bases for ethics and a philosophy of values.

The concept of synthesis logically compels the admission that in the beginning there was chaos, for if there were unity the notion of synthesis is a pure redundancy. The assumption of an original chaos in the universe at large, or in the beginnings of mental life, or at the beginning of a learning process, is a totally unmanageable concept. One cannot obtain unity from chaos. There is no such thing as relative unity; unity is absolute with respect to any given set of conditions; and as conditions change, wholes change phenomenologically, not functionally. The laws pertaining to the problem of organization are universal. Thus synthesis, which many writers have attempted to employ in deriving one kind of unity from another, in no way frees the systematic

psychologist from the original evils of association and attention psychology, for it harbors all the assumptions and working methods of pure atomism. To assert that one process is relatively unified and another more so, is to apply to both a double standard. It supposes that the criteria of unity in the one case are conditions then non-existent with respect to the process. This is the basic error in the interpretation of certain forms of behavior as 'trial and error.' The same mistake is not eliminated when more unity is said to be derived from less, unity from synthesis, for unity is absolute for a given set of conditions, and synthesis plays only the rôle of a subtle *deus ex machina.*

The only manageable concepts in psychology are the laws of dynamics, or in other words, field forces, and the laws of dynamics are neither mechanistic nor vitalistic. They are organismic. They are laws of organized systems of measurable energy whose organization is axiomatic. Wholes are fundamentals, even as phenomenological configurations, and their unity is quite as inviolate in respect to time as in respect to space. The conventional theories of memory that attempt to account for a subsequent process in terms of an antecedent (trace), and the notion that we learn by experience, all violate this principle. No matter how complex a process, *it* cannot be accounted for in terms of its parts or temporal sequences; one phase is not an explanation of the next. What shall be explanation and what shall be description depend entirely on the scope of the problem. The extent of our knowledge of any process or subject depends upon the comprehensiveness of the unified whole which can be observed as the factor that conditions that process or object, and gives to it its properties. This is the sense in which structural analysis leads to fictions; and this is the sense in which all knowledge harbors artifacts to some degree or other. The evolution of scientific thought is a process by means of which artifacts are reduced to a minimum. In the light of our present knowledge, artifacts can be reduced to a minimum only when parts are treated, on the one hand, in the light of their membership character in larger dynamic wholes and when,

in turn, they are treated, if segregated, as *complex* dynamic wholes in their own right. Atomism increases the artifacts by multiplying the elements entering into the chaos. The organismic approach reduces the artifacts in proportion to the comprehensiveness of the whole selected as the descriptive unit.

Such are the contributions of *Gestalt* psychology, implicit and explicit, and yet we hear all too often that its fundamental position has long been held. Had it been held, our psychology would have been revolutionized long ago, whereas, on the contrary, it has remained static, systematically, for 200 years. The contributions of *Gestalt* psychology force a re-definition of practically every psychological fact; a complete turning of the tables in our psychological thinking. The reason? We have an entirely new type of unit, both descriptive and explanatory; a whole as a principle and as a cause and not as an effect or mere fact.[5]

2. *The necessity of system.*—Every psychologist has a system. It avails one nothing to plead that he is neutral with respect to a system, or that he has no system. It is impossible to think except systematically. The only difference between psychologists in this respect is that some of them admit their systems and some do not. In fact many there are who seemingly neglect the logic of the very system in which they are thinking, for the same reason that the man on the street does not know that his view of the universe

[5] The unity of wholes, previously admitted as facts, was never understood by John Stuart Mill, by Wundt, by Titchener, by Mach, by James or even by Ehrenfels; it was not, nor could it have been, employed as a principle. James, Titchener, Ward, Stout, all tried hard to avoid atomism in its earlier associationistic and apperceptional form, but they only substituted one form of it for another, and quotations so often erroneously offered to prove the contrary will be submitted in the next paper. These systems were camouflaged atomisms as certainly as psychophysical parallelism and the double aspect theories were camouflaged dualisms.

Evidence will be brought forward later to show that the progress made through the centuries of atomism was that of running it into the ground without seeing the way out—*a distinct service*, but a negative one and not logically an approach to *Gestalt* psychology. Indeed it is only by coincidence that phenomenology is linked with the name of *Gestalt* and it is only by virtue of a temporal relation to *Gestalt* principles that it was saved from being the worst sort of atomism we have ever had. Potentially, phenomenology, as such, is sheer structuralism over again.

(material *versus* spiritual) is a three-hundred-year-old philosophy parading as common sense. And thus, an author insists that he belongs to no school—he dislikes them—and unwittingly deceives the naïve reader into believing that here at last is broadmindedness, while the truth of the matter is that the *author has accepted the psychological mores of the day oblivious to their worn out but bristling logic.* Familiarity breeds contempt for the logical and systematic aspects of psychological points of view as well as in other situations in life. One is reminded of the fundamentalist who so vigorously protests against the determinism of science, quite unaware that he is giving to himself and to others far less freedom than science would give him. Again the proof resides in print and will be submitted later. Furthermore, psychology has not yet reached the stage where differences in point of view are not of necessity incompatible; and once more the proof of the pudding is in the eating. Eclecticism harbors myriads of self-contradictions, and is in itself a false position. Systematically there can be but one basic point of view in science—one correct view—and the only view that will iron out present contradictions is the vision of a new and larger whole that gives consistent meanings to all the facts of observation. That larger whole must abandon completely the logic that has gotten us into the difficulties. The old and the new will not mix. Efforts to mix them result in the most pitiful of confusions and self-contradictions. Eclectic efforts of today abound in them and again the naïve reader is the sufferer. So is the eclectic himself. Again and again we read the declaration of someone that he has long ago abandoned, for example, the constancy hypothesis. But he abandons it only by intent and continues to use it, perhaps in a more subtle form, the more serious because of its subtlety.[6]

[6] The senior author was conferring only a few months ago with a student who was presenting the results of some independent research. The author threw up his hands and declared that the results were impossible. 'But they confirm your own theory,' was the student's protest; and not until several others had joined in the protest did it become evident that the author was harboring the constancy hypothesis with respect to this problem in as raw and crude a form as was possible to conceive, in spite of the fact that he had prided himself on having thrown it all overboard long

Such is the unnoticed logical force of tradition, a force that is no less potent among scientists than among laymen, for science is a form of social behavior as dominated by mores as the conduct of the man on the street. Heresies are as frequent in science as they are in religion; to challenge the *mores* brands one as an enemy of the truth in science as well as in politics. And it will not do to say that the psychological difficulties of today arise out of language. When one changes his ideas, he changes his words without difficulty and not until then. Then, indeed, he insists upon a change in words.

3. *Fallacy of regarding a new system from an old standpoint.* —There is a fallacy in contemplating a new system from an old standpoint. In proportion to the extent that old ideas are not repudiated deliberately for their false logic, the meaning of the new will be missed. Obviously when the falseness of the old ideas is not appreciated and when the new is defined in terms of the old, there is nothing that seems new; the real issues remain uncomprehended; there is no problem; and the attacks upon the old point of view are naturally characterized as fictitious—the building up of straw men to tear them down again. A most curious anomaly of reasoning issues at the present time from this set of conditions; the comedy of a principle employed in criticizing the configurationists, borrowed from them but regarded as indigenous to the old point of view. In the same breath the new point of view is degenerated into the old by the critic himself and the product used as a whip. Thus Lund [7] writes, "the *Gestalt* psychologists in declaring that experience is not a composite of sensational or behavior units but a composite of 'wholes,' 'units and sub-units,' called *Gestalten* would appear to have substituted one form of atomism for another." There could be no worse a garbling of the *Gestalt* position, which explicitly declares that there is never a *composite* of anything or that wholes are ever *composed* of parts or units. The substitution

ago! We are not therefore accusing others of doing something that does not happen in our own laboratory; but since we are frank enough to admit it, perhaps we can be pardoned if we point out similar mistakes in others. The atomistic character of eclecticism will be demonstrated in a later paper.

[7] L. H. Lund, The phantom of *Gestalt*, *J. Gen. Psychol.*, 1929, 2, 309.

of one atomism for another is purely in Lund's mind; he is still thinking in terms of atomistic, psychological mores. The configurationists never declared that experience is a composite of wholes; nor have they ever implied it. The necessity of starting with abstracted parts is explicitly admitted by the configurationists, but the consequences are just as explicitly faced, for they insist that by starting with parts one can say nothing about the wholes from which they came. The parts must be investigated as wholes, with reference to their own parts, and one cannot reason from parts to the whole. Such are the consequences of reading the old into the new.

In the same paragraph, Lund attempts to tell the configurationists something they did not know: "However, to understand human experience we must have clearly before us *not* a set of *Gestalten* [this is Lund's own problem, not the *Gestalter's*], nor a complex of sensations and images, nor a system of reflex patterns—conditioned or unconditioned—but a living organism adapting itself to a complex and changing environment, an *organic* whole with a great many interrelated functions articulated and inseparable except under artificial analysis." [8] Is this latter statement Lund's or is it inaccurately borrowed from the configurationist in an effort to find something to criticize? Let the printed facts answer this question. We read this statement: "We cannot agree then that sensations are not components of the experiential sequence." [9] "However, in the learning of a new language are meaningless sensory elements not acquiring meaning?" "It is a matter of everyday experience to notice how associated sensory elements tend to lead over into the same reactions . . . come to *mean* those objects with which they are associated." [10] "The awareness of movement itself, or the perceptual response, is made up of all the associative responses formed in connection with the activity. . . ." [11] "And to say that the constructs of experience, perceptual or

[8] *Op. cit.*, 309.
[9] *Op. cit.*, 311.
[10] *Op. cit.*, 317.
[11] *Op. cit.*, 320.

overt, are not built up gradually from original reaction units is to suppose that physiological constructs, corresponding to the phenomenological, also have existence from the very beginning, which is equally untenable." [12] And thus the primacy of organization and wholes is explicitly denied; we do not, after all, have 'an *organic* whole with a great many interrelated functions articulated and inseparable except under artificial analysis.' The psychological wholes which Lund declares are untenable have been discovered in adult behavior, in the perceptual processes of children and in the behavior of infants. The physiological constructs which he declares are equally untenable have been discovered by Coghill,[13] Tracy,[14] and others and long ago by Child [15] and his co-workers. Because he is thinking atomistically, Lund does not understand the meanings of the words he uses. The assertion, 'Many interrelated functions articulated and inseparable except under artificial analysis' can only mean that under all circumstances the whole (and only the whole) conditions the activities of its parts, for that is the only way relations can be envisaged. Lund himself denies this principle after having stated it. "It is no more true," Lund says, "that the character of the constructs influences the character of the parts than the reverse." [16] His general position, implied in the phrase '*organic* whole' which he uses, is systematically the reverse of the organismic one, for all processes according to him (following Hollingworth) are matters of redintegration *where parts condition wholes.* The principle of redintegration, thus portrayed, violates all the laws of dynamics and is a physical impossibility. No part, in terms of dynamics, can influence another part except under the laws of a whole and as the properties of the parts are derived from that whole. *Thus the configurationists are*

[12] *Op. cit.*, 321.

[13] G. E. Coghill, Anatomy and the problem of behavior, New York, Macmillan, 1929.

[14] H. C. Tracy, The development of motility and behavior reactions in the toadfish (*Opsanus tau*), *J. Comp. Neurol.*, 1926, 40, 253–370.

[15] C. M. Child, The origin and development of the nervous system, Chicago, Univ. Press, 1921.

[16] Lund, *loc. cit.*, 311.

again accused of doing something they never did, and that is not all; they are admonished by Lund to do something which they have always been doing but which Lund himself fails to do.

A similar situation in general may be found in Boring's [17] attack upon the *Gestalt* movement. Boring applies the criteria of the second historical type of whole to the configurational whole, unmindful of the radical change that the entire logic of the descriptive unit has undergone, and with it the logic of all interpretative principles. Again the printed words speak for themselves. In A History of Experimental Psychology,[18] Boring accepts Aristotle's declaration that the soul is a totality without parts and proceeds in the next two pages at least six different times to contradict this statement in Aristotle's own terms. The real logic of Aristotle's psychology, which was quite as atomistic as Plato's before him, is missed. For Aristotle, unity of the soul was not a systematic, psychological issue, but a religious one. He had no better notion of unity beyond structured, phenomenological simplicity and homogeneity, than had primitive man. Primitive man would have said that the soul contained no parts. The denial of parts (members) is not the essence of the new totality, but of the most primitive kind of all. Aristotle did not see the latent inconsistency and Boring has missed it. Aristotle compartmentalized his soul as a whole from his analyzed soul quite as effectively as the modern layman compartmentalizes his soul from his analysis of behavior. There is no suggestion of *Gestalt* principles in Aristotle. Aristotle's hierarchy of *forms* were logically distinct soul-atoms that summated in a purely mechanistic fashion, and *even underwent subtraction at death.* In the noëtic soul two distinct, discrete faculties—discrete, separately functioning parts—were distinguished. There are numerous other evidences of atomism. The soul is localized in a certain part of the body; there are five discrete senses (which Boring thinks is still good doctrine although he implies that he no longer adheres to the constancy hypothesis of which the

[17] E. G. Boring, The *Gestalt* psychology and the *Gestalt* movement, *Amer. J. Psychol.*, 1930, **42**, 308–315.

[18] Boring, *loc. cit.*, 156.

doctrine of specific senses—a particular sense quality for a particular form of stimulus—is proof to the contrary); and again, Aristotle separates off sensing from judging. The whole is after all built from its parts, because cognition proceeds from sense and experience; and sensations and thought follow laws of association. Configurational psychology repudiates each of these points; and yet, according to Boring, it would in its central dogma go back to Aristotle. Aristotle's *forms* of the soul were, as he used them, fixed structures; *form* for Aristotle was anything but the *Gestalt* notion of form. It was not a dynamic balance, a fluid phenomenon. Had it been, nothing would have been said about the inclusion of one form in another; all would have had a common, basic, total pattern and the differences would have been of degree of differentiation only. What Aristotle did mean by form is quite apparent. Form, with matter, was his statement of the structure-function dichotomy, a distinction so general that it will not, as such, distinguish one system, an atomistic one, from another, an organismic one. All systems must face this issue. Aristotle's atomism *follows after his answer to the structure-function question.* Accordingly, Boring is content that the central dogma of *Gestalt* psychology is just the fact of totality; which is not the kernel of the doctrine at all.

The same fallacy reveals itself in a still more explicit fashion, in Boring's recent attack upon the *Gestalt* movement.[19] He complains that the introspectionist whom Köhler is attacking in his book Gestalt Psychology,[20] is a hypothetical person, and that the behaviorist, also attacked, is a myth. Thus the *Gestalt* psychologist erects straw men just to have something to tear down, and it all comes about in the effort of a movement to perpetuate itself. In reality, Boring declares, "He [Köhler] is not making of organization a formal, synthetic principle. He is saying: let us keep to observation; whatever experience insists upon is scientifically

[19] E. G. Boring, The *Gestalt* psychology and the *Gestalt* movement, *Amer. J. Psych.*, 1930, 42, 308–315.

[20] W. Köhler, Gestalt psychology, New York, Alfred Knopf, 1929.

true. Well, that faith is nothing new. It was the faith of Wundt. If Köhler thinks that Wundt failed, it was not because he had the wrong faith. Schumann had the same faith, and he described *sensory organization* in terms of *attention*. The difference here is one of words only. *Gestalt* psychology does not like *attention* as a concept because it is vague. *Sensory organization* is equally vague. We shall get nowhere until we have a successful hypothesis about the organization. Titchener had the same faith. In order to get rid of the vagueness in *attention* . . . he called it *sensory clearness* and much later *attensity* . . . Wundt, Schumann, Titchener and Köhler have all faced the same problem, have appealed to the same faith for its solution, have achieved different descriptive terms for the phenomenon, and have gotten almost nowhere with the answer. . . . After introducing concepts of the self and of purpose, Köhler proceeds to show that *insight* is the organizing principle by which organization is dynamically constituted in time. Köhler . . . lacks even a physiological hypothesis to inflate its frail fabric . . . *unconscious inference*, *apperception*, *attention*, *determining tendency*, *libido*, *instinct*, *purpose*, *drive*, *conation*, and now *insight*. I cannot see that we are one bit further along." [21]

Boring does observe (which is more than most critics do) that Köhler is not making synthesis a formal, organizing principle, but he misses the reason why. Köhler is saying much more than "Let us keep to observation; whatever experience insists upon is scientifically true." His is certainly not the faith of Wundt, who believed in synthesis as a creative principle; not the faith of Titchener who ruled out functionalism altogether in order to get rid of the problem; not the faith of Schumann who substituted for synthesis another vitalistic principle, namely, attention. None of these men had the faith that organization was primary, that wholes came first in *principle*, otherwise, they would not have spent all their time trying to account for wholes when the problem was fictitious. Köhler's is a different sort of faith altogether,

[21] Boring, *loc. cit.*, 313–315.

based upon an entirely different logic; he is not seeking for a principle that will account for organization when organization is axiomatic. His is the faith that organization can and must be used as an explanatory principle; as cause, not effect; as something primary, not derived. The whole that Wundt, Titchener, and Schumann tried to account for, Köhler employs in accounting for the parts. When Boring declares, therefore, that sensory organization is a concept equally as vague as attention, he again proves that he has missed the point altogether, namely, that science has evolved to the stage of employing a logically new and distinct type of descriptive unit. Sensory organization is one way of naming the new descriptive unit, hardly vague, when the dynamics of this unit are a matter of demonstration, daily. They are demonstrated every time an object falls; in every weather prediction; every time we turn on an electric light; in every brook and river; in everything that grows; in every goal-activity of animals and man; every time an eye moves; in every perception and recall. The difference is not one of words only; it is one of words with a whole new framework of logic behind them, that gives them an entirely new meaning.

"We shall get nowhere until we have a successful hypothesis about the organization," Boring asserts.[22] If Boring's statement is correct, science can get nowhere; concepts of gravitational stress, electric potentials, atmospheric pressures, physiological gradients, disturbances of equilibrium, releasing of potential into kinetic energy, the law of least action, the laws of thermodynamics, any law having to do with balance, are worth absolutely nothing. Boring is repudiating the basic principles of all science in a single swoop. It is not evident to him that configurational theory is based on the organismic character of the laws of dynamics and their universal nature, on the relativism in the two kinds of complexity, functional *versus* phenomenological, and on the evolution of a new descriptive unit in science.

Köhler and his predecessors did not face the same problem

[22] E. G. Boring, The *Gestalt* psychology and the *Gestalt* movement, *Amer. J. Psychol.*, 1930, 42, 314.

at all, except as we apply to the evolution of scientific thought a double standard, that of the past and that of the present, as confusing and as unjust a thing to do as to apply the double standard to the learning process and to assert that some learning is trial and error while other learning is not. No, they did not face the same problem. Wundt, Titchener, and Schumann faced the problem they created for themselves, the problem created by the first two types of historical descriptive units, the problem of deriving unity out of chaos. Köhler is doing the same thing only to the extent that he is facing a problem he has created for himself by his fundamental assumptions, but it is a different problem. Any problem is created by the assumptions one makes in the beginning. Since these were altogether different in the two cases the problems are altogether different. They have not, then, appealed to the same faith for the solution, but to opposite faiths. Did Köhler get nowhere with the answer? If one is not convinced already let one watch for a time the progress of science, including psychology. Perhaps on close inspection he can see changes occurring very rapidly at the present time. But let no one be so misguided as to claim that he has held the new view, at least implicitly, for forty years.

Boring further misunderstands the whole issue between *Gestalt* psychology and its past when he interprets Köhler's insight as an 'organizing principle.' [23] Insight denotes unity, it creates no unity; insight describes the unitary, whole-character of behavior and is quite as observable as 'boiling' is observable when one watches a kettle of water over a flame. It is a name to describe a given situation as a whole, and it pertains to the phenomenological field property of that whole. It is no more intangible and quite as potent as the field property of a gravitational system, or a physiological gradient or an electrostatic field. The most important situations in physics are not as wholes measurable nor are they palpable, nor are they in psychology. Unconscious inference, attention, determining tendency, libido, instinct, purpose as formerly defined and used, and conation, were one

[23] Boring, *op. cit.*, 314.

and all mechanistic and vitalistic concepts conceived and developed in an atomistic framework of logic. Insight is no more in this category than the notion of a given potential which is not self-defining (it is defined in terms of a system as a whole), not a segregated entity, is in the same category as the conventional atom whose dynamic character was self-defining, something inherent in the atom, alone. And finally, our critic must have had access to the works of Child and Tracy, if not of Coghill, which gives Boring precisely the physiological hypothesis to 'inflate the frail fabric' [24] of insight that he insists is lacking.

4. *The problem of temporal continuity.*—The fourth major problem certain to be misunderstood by anyone harboring the least vestige of atomistic thinking, is the problem of temporal continuity. A *Gestalt* or configuration, is a unit in time as well as in space. Any attempt is atomistic that tries to account for memory, for example, in terms of past experience (redintegration, trace), or for the growth process in terms of previous stages of growth, or for any continuum in terms of temporal phases of that continuum, or for the present in terms of the past. The new descriptive unit, a whole, contains, with respect to its parts, both past and future. This is another way of saying that for wholes there is only one time, namely, the present. Past and future are concepts applicable only as one thinks from a whole to its parts. It is not applicable when one thinks from part to part. Mechanistic thought attempts to account for the present, atomistically, in terms of the past, alone. In organismic logic the present is a product both of the future and of the past.

Consider the simple case of a falling apple. Suppose the apple is on its way to the ground. The center of the earth, and the distance yet to be covered, are ahead of the apple in time and in space; in other words, they are the apple's future. As everyone knows, the center of the earth is an essential factor in explaining the apple's fall, for the fall is a process of resolving stress, through kinetic energy, toward balance

[24] Boring, *loc. cit.*, 315.

or equilibrium. (We are here relating the apple dynamically to the earth, for convenience leaving out the other side of the picture, the relation of the earth to the apple. But the same principle holds when the picture is reversed.) The apple, then, is responding to its future. The future, with respect to the apple, is conditioning what the apple is doing in the present. In other words, *determining conditions surround a given event in time as well as in space.* The structurization of any *Gestalt*, in time, cannot be understood until the *Gestalt* is clearly envisaged as a temporal as well as a spatial unit, conditioned by its future as well as its past, and *by both at the same time so far as the Gestalt is the point of reference.* Later the application of this logic to the problems of memory, mental development and learning will be considered in connection with recent criticisms of *Gestalt* interpretations.

5. *Fallacy of the double standard.*—This relativity of time as well as of space leads to a consideration of the double standard mentioned in the preceding section. It is a common fallacy of atomistic thinking to attempt an explanation of one thing in terms of conditions that apply not to that thing but to another. The old law of parsimony, as applied to problems in behavior, committed this fallacy, for in terms of the law as stated by Morgan, for example, it is presupposed that an account of animal behavior is more adequate when given without reference to intelligence. The effort was to avoid anthropomorphism. But the procedure of avoiding anthropomorphism committed the error the psychologist thought he was avoiding. It requires more anthropomorphism to evaluate a performance as trial and error, for the *standard* of evaluation must be a performance requiring *higher* intelligence. In other words, a person knowing the solution of the problem evaluates an animal's behavior in terms of the conditions as he sees them. These known conditions have nothing to do with the animal's performance. The conditions that account for the performance are different but function after the same principles as operate in human behavior. One might just as well say that a cyclone, looking as a steady

breeze, ridicules the breeze for being so stupid and impotent, when, after all, both are obeying the same laws of dynamics under differing sets of conditions. The difference between human and animal behavior is phenomenological, not a difference in principles. But atomistic thinking assumes that the principles are different; that they must keep pace with the evolution of phenomenological complexity. That animal and human behavior follow the same principles is no more striking than the fact that the atom contains, in principle, the laws of the whole Universe. The fallacy of the double standard vitiates the bulk of our account of the learning process; it is creating untold waste in our educational program from the pre-school to the graduate school; it invalidates much of the interpretative history of psychology. The two situations are quite similar for the history of science is an outline of a racial learning process. It leads to the erroneous notion of common elements in successive phases of a developmental process; it is the artificial reading of the old into the new and the new into the old.

6. *Relation of words to concepts.*—A sixth source of confusion is the erroneous belief that changing words in depicting a situation does not change the logic of the situation. As a matter of fact man uses the same words so long as his ideas do not change, and a change of words is a sign of a change in ideas. Words are defined by the logical framework of thought in which the use of the word emerges. Until one knows what that framework is, therefore, he is treading on dangerous ground when he asserts, dogmatically, as has been done so often, "all you have done is to change a word." Of course this would seem true to a person whose thinking has not changed. But sometimes more arguing than seems necessary is required to convince a critic that his ideas have not changed enough to give him the right understanding of new words. Anyone who, at the present time, declares that *Gestalt* is an *empty* concept, or that organization is a mystic word, or that insight is a vague term as opposed to reflex (what could be more vague than the term reflex in the light of our present knowledge?) or that tension is only another

name for instinct, or that *Gestalt* was dead before it started, is still hopelessly enmeshed in atomism. Any attack upon configurational terms from the atomistic point of view merely lays the critic open to a rejoinder whose significance and potency can be measured by no less a yardstick than recent revelations in all science from physics through biology to psychology. The new terms have behind them an entire logical framework. Such an ignoring, if not a contempt, of an evolutionary process so profound as a fundamental change in the descriptive and logical unit in scientific thought is, to say the least, misguided, pretentious and foolhardy.

7. *Difficulties of abstracting ideas from their context.*—Much confusion is caused by taking statements out of their context when the context itself is not evaluated and permitted to give the statement the meaning which rightfully belongs to it. Usually the critic who abstracts these quotations from their setting misses the fact that they do not demonstrate what he intends them to demonstrate. For example, Squires [25] quotes from Wundt to the effect that processes are of a 'composite' character, "each of these processes is itself a more or less composite whole." Just what Squires is attempting to prove by this quotation is difficult to comprehend. He asserts that the *Gestalt* writers unfairly claim primacy in an emphasis upon the uniqueness of wholes; but this proves nothing but an error on Squires' part for it is irrelevant to the issue. The issue, as we have already pointed out, is 'What to do with this uniqueness of wholes.' The *Gestalt* claims were not read with an eye to their context, nor was the statement of Wundt. For, 'uniqueness of wholes' in Wundt means uniqueness derived from creative synthesis, an atomistic assertion, while 'uniqueness of wholes' in the *Gestalt* literature refers to 'primary uniqueness,' to an organismic principle based upon opposite assumptions. Further, Squires quotes Külpe as follows: "*real isolation*,—the actual experience of one single sensation, for instance,—*can never take place*." [26]

[25] P. C. Squires, A criticism of the configurationist's interpretation of 'structuralism,' *Amer. J. Psychol.*, 1930, 42, 136.

[26] Squires, *op. cit.*, 137.

What of it? It was never claimed that artificiality of abstraction was unique to the configurationist's position. Again, taking statements out of their context leads to a missing of the point. *The uniqueness of the Gestalt position is in the refusal to do anything with the abstractions* after they have been made, other than to treat them in their *own right as segregated wholes.* Külpe said nothing to the effect that a sensation, once abstracted, always remained so; instead, he attempted to reconstruct his whole from them; no one prior to the organismic movement ever said that a reflex, once emerged from mass-action, always remained so. Instead, the reflex, a confessed abstraction, was used as the unit which, integrated with like units, constituted the total pattern of behavior. A part, once emerged, remains so. This is the organismic contribution to the problem of abstraction, a contribution quite foreign to the systems of Wundt and Külpe.

The same fallacy of criticism lies in reading into Mach and von Ehrenfels a *Gestalt* concept simply because they emphasized emergents and the unanalyzable character of *form.* The *Gestalt-qualität* of Ehrenfels belongs to a bygone age when atomism prevailed, for the *Gestalt-qualität* was, by confession, an element of consciousness on a par with other elements like sensory processes and imageless thoughts. There is nothing in the logic of the *Gestalt-qualität* any more like the whole of the configurationist than like the unanalyzable whole of primitive man.

We come to Titchener. Squires quotes this author at length in an effort to show the closeness of his logic to that of the configurationist. "Mind has not grown by aggregation of sensations." [27] But nowhere do we find a statement as to how mind does grow. *On the contrary we find nothing but atomism.* The perception that is more than the sum of its parts is a group, a composite or a synthesis of sensations. Perception, in fact, is 'sensation plus image,' yet we are told, contrawise, that it is not a summation! The essential thing about a perception is its meaning, says Titchener; we

[27] Squires, *loc. cit.*, 137.

are told also that meaning is not a legitimate psychological datum! We are informed that perception is given, and again that it is not, *for only processes are given*, and the entire system is based on the latter not the former statement. Titchener got no farther than Aristotle in his logic, for his genetic whole, like Aristotle's soul, is safely compartmentalized against description or analysis by being ignored. Meanings are derived through the formation of contexts. Again, misunderstanding relative to the wholeness of perceptions 'is fatal to the student of psychology, for it means misapprehension of the central psychological problem.' [28] Obviously we are never told explicitly what this central psychological problem is; but the context of the book from which these quotations have been taken does tell us. It is twofold—the atomistic derivation of a whole from its parts and the ruling out of function. The latter position is what prevented Titchener from seeing the error of his distinction between the analytic and genetic unit.

Squires goes on, "clear and unequivocal recognition is accorded the phenomenological actuality of wholes, *Gestalten*. . . . Sensation is treated as an analytic not as a genetic concept . . ." *et cetera*. "It would appear that the configurationists have not noted these facts," Squires declares.[29] On the contrary they have noted these facts and a lot more, namely, that the phenomenal actuality of wholes, long ago recognized by structuralists, was recognized from the time of the Greeks; that this is not the issue between the old and the new psychology; that a distinction between the analytic and the genetic unit is fallacious; that Titchener's use of the analytical unit instead of the genetic one (to employ Titchener's own logic) makes him just as much of an atomist as if he did not draw the distinction, because his genetic unit means only that wholes are first in fact, not in principle. The genetic unit is, in Titchener, nothing but a statement of the fact that 'before we begin to analyse, all of mind is there,' no more significant a statement than that 'before we begin

[28] E. B. Titchener, A textbook of psychology, New York, Macmillan, 1910.
[29] Squires, *loc. cit.*, 138.

to observe nature the whole universe is around us.' No; before one approaches, or begins to approach, the configurationist's position he must acknowledge something aside from the phenomenological priority of wholes; he must acknowledge the functional priority of wholes, which is the essence of the *Gestalt* position.

There are many other errors in Squires' critique, as for example, his conviction that *Gestalten* are entities, and that Wundt and his followers never taught that mind is the result of bundling together of psychic atoms. These are sheer misstatements of fact, again traceable to the premature isolation of specific statements out of the wholes from which they derive their meaning. A more detailed consideration of these errors will appear in a subsequent paper.

8. *Relation of facts to theory.*—Atomistic logic leads to the false supposition that a fact can be stated without also stating, implicitly, a theory. Generally the theory is actually stated in putting the fact into words. The truth of the matter is that no fact can be stated without committing the declarer to an entire systematic position. Even the common-sense assertion, for example, that a piece of chalk fell to the floor, commits the declarer to an atomism that, to be sure, he may not have intended, but an atomism, nevertheless; for the phrase acquired its meaning and is still used in a framework of assumptions that common-sense accepts as fact. The assertion implies that here we have a piece of chalk, a movement of falling, and the floor, the only factors having anything to do with what happened, and each fact is a discrete entity. Any uninitiated person would swear that this was all there is to it. Stand some dominoes close together in a row. Tilt the first one over and the whole row falls down. Common sense assumes that each domino knocked its neighbor down. But, in reality, it did not knock its neighbor *down* any more than it caused it to fly *up*. The assertion implies an atomism, causality between the parts of a situation; in fact, the statement explicitly declares such a causation. In psychology any naïve declaration presupposes an atomistic logic in precisely the same fashion. We shall have occasion

to treat many of these in detail, from writings that claim to have avoided atomism.

9. *The two-way argument.*—There are many arguments that work both ways and are therefore invalid in discussing an issue. A typical example of such an argument is the mutual accusation of opposed schools that the other cannot introspect. Boring employs a similar type of argument when he resorts to an emotional appeal. The *Gestalt* movement is erecting straw men to have something to fight, for it thrives on opposition; then the straw men 'go down to an easy defeat at the hands of their doughty creators.' [30] The configurationist writes out a 'John Doe warrant for the arrest of somebody.' Köhler is 'haunted by the ghost of a psychologist long since dead.'[31] We might also mention comments on cavalierism, knights advancing in armour, charges of false dialecticism, anthropomorphism, mysticism, scholasticism, dogmatism, narrowness, quibbling and much ado about nothing. Naturally the human aspect cannot be divorced from argument, but much valuable time and precious space could be saved if such arguments were reserved for the colloquium and the smoking room where humor, mirth and emotional *repartie*, are more appropriate, and the printed argument were so selected for its logic and factual content that it would speak for itself impersonally.

It would be an easy matter to return the emotionalism and to present equally as good a case for Boring's erroneous position. If a movement must perpetuate itself, like the church, by imagining false enemies, so must an old system, resisting disintegration, defend itself to the last ditch by all manner of artificialities, not least among them the false alibi that "why, I made this contribution myself, you didn't." There is a well recognized form of social behavior known as identification, wherein one imitates a person of popularity and prestige, perhaps all the while protesting against that popularity after a sour grapes fashion, but at least unmindful that the underlying motive is an effort to take on some of

[30] Boring, *Amer. J. Psychol.*, 1930, 42, 310.
[31] Boring, *loc. cit.*, 311.

the grandeur and prestige of the imitated. One resents a slurring remark about his house or his community or his dog, because he identifies himself with them. They are *his*. Perhaps one becomes impatient in debate sometimes, in science, for the same reason that he becomes angry in an argument, merely as a round-about admission of defeat which one is attempting to hide. We must not be misunderstood. We are not making these charges. We are merely warning our critics that an argument that works both ways is not good, for, very likely, it is based neither on facts nor sound logic. As far as this issue is concerned, emotion is irrelevant.

Finally, we find in the array of recent criticisms of *Gestalt* psychology, on the whole, this interesting paradox:

(1) There is nothing new in *Gestalt;* we have held these ideas all along; they are the venerable contributions of the masters of the past whom we have always followed.

(2) *Gestalt* is empty, vague, mystical, a phantom, a grand and glorious word, a maid of all work, a panacea for all evils.

(3) If *Gestalt* is just the venerable, respectable psychology all over again in new clothes, then the targets of all this indifference, sarcasm, dogmatism, ridicule, even contempt, are, in terms of the critics' own position, the critics themselves, and the very psychologies which they are defending. The myth, the ghost, the straw man, comes home to roost!

[MS. received November 10, 1930]

SOME WEAKNESSES IN THE EXPLANATION OF HABIT FIXATION AS CONDITIONING

BY J. M. STEPHENS

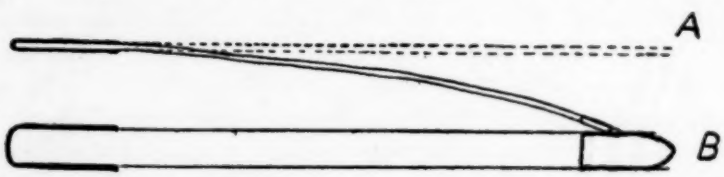
The Johns Hopkins University

This paper discusses the following:

Several possible interpretations of the conditioned reflex; the value of the explanation of learning in terms of the conditioned reflex as compared with an alternative explanation (the law of effect or principle of retroflex); the possibility of extending the law of effect to account for the identity of the response of the conditioned and unconditioned reflex without recourse to the drainage hypothesis.

I

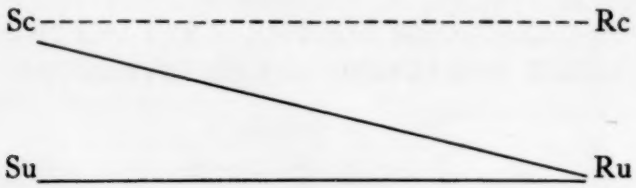
The growing vogue of the concept of conditioning as an explanation of selection in learning leads one to ask just what facts are meant by conditioning. Apart from backward conditioning which, in spite of some recent work,[1] seems hardly established as yet, the term may refer to several rather different phenomena. We may think of the general idea of conditioning as something after the following style: If a cannon and a rifle were pointed in parallel directions, the rifle pointing at A and the cannon at B, and if each were to expel simultaneously a magnetic projectile, we might suppose



that the cannon ball might attract the rifle bullet and that the latter might swerve toward B. This idea of the stronger

[1] St. Clair A. Scwitzer, Backward conditioning of the lid reflex, J. EXPER. PSYCHOL., 1930, 13, 76–97.

of two simultaneously operating nerve tracts draining the energy of the weaker is often expressed by the formula:



In this schema neural energy is released at Sc (by the ringing of a bell) and at Su (by the presentation of meat) at the same time so that the passage of energy from Sc to Rc (say attempt to run away) occurs while energy is passing from Su to Ru (salivation). The passage of the impulse from Su to Ru, being 'dominant,' is supposed to draw the energy released by Sc away from Rc to Ru. Immediately questions arise. Could the operation of the dominant impulse drain the energy of Sc–Rc after the latter had reached Rc? In the case of the cannon-rifle analogy it obviously could not. In the case of neural energy we can hardly speak of it as impossible, but if Sc–Rc has already operated before Su–Ru occurs, the idea of simultaneously operating neural forces does not hold, and the objectionable 'back-kick' of the law of effect is also implied in conditioning. Now Pavlov holds[2] that this very condition is essential.[3, 4] Unless Sc occurs before Su, conditioning will not take place. Now if Sc occurs before Su, the energy released at Sc must be going somewhere (law of dynamogenesis) at a rate sufficient to produce $2\frac{1}{2}$ reflexes per second before Su occurs. That is to say, 400 sigma after the occurrence of Sc some response will likely have taken place—the bond Sc–Rc (the latter being any response to Sc) will have operated. Then if Su is 400 sigma later than Sc the simultaneous energy flux would not hold. Since a delay of at least 1000 sigma is the usual thing and some delay absolutely necessary (and surely any delay

[2] I. P. Pavlov, Conditioned reflexes, Oxford Univ. Press, 1927, pp. 49–62.

[3] St. Clair A. Scwitzer (reference one above).

[4] C. L. Hull, Simple trial-and-error learning, a study in psychological theory, Psychol. Rev., 1930, 37, 241–256.

controlled by ordinary means would be at least 400 sigma) to conditioning, we must look to some other hypothesis than drainage by simultaneously operating tracts to explain that phenomenon.

If all our data in the typical conditioning experiment were recorded on a kymograph we would not have the familiar

Sc–Rc
Su–Ru

but (representing intervals of 200 sigma by vertical divisions)

Time (in seconds) elapsed after occurrence of Sc

0	$\frac{1}{5}$	$\frac{2}{5}$	$\frac{3}{5}$	$\frac{4}{5}$	1	$1\frac{1}{5}$	$1\frac{2}{5}$
1	1	1	1	1	1	1	1
Sc		Rc			Su		Ru

If this happens often enough we may find that instead of Rc we get some other R and that this may eventually come to be Ru. If this Rc changes to Ru we call the phenomenon conditioning. If it changes to Rx (*i.e.* a response different from Rc and not Ru) we call it learning. Thus if the child seeing the flame (Sc) reaches for it (Rc) gets burned (Su) (after Rc has taken place) and withdraws his hand (Ru) and later withdraws his hand immediately after seeing the flame, we call it conditioning. If, however, the sight of the flame had led to blowing out the flame and this to a spanking and this to violent crying and the next sight of the flame had led to leaving it alone, we would not have a case of conditioning. The criterion of conditioning is the identity of the response which is finally selected to follow the conditioned stimulus and the response originally following the unconditioned stimulus. If these two responses are the same we have conditioning, if not, we have ordinary trial and error learning.

II

It is the purpose of this paper to show that such a criterion is purely arbitrary, that the similarity of the selected and unconditioned responses is merely an incident in conditioning which otherwise fits in with the trial and error schema. It is

also proposed to show that the conditioning explanation of learning will not hold in its own field and that the identity of the responses found in conditioning is a logical result of the operation of the trial and error schema. That is to say, that trial and error learning is the general rule, the addition to which of certain incidental conditions will result in the phenomenon of conditioning.

TABLE I

Situation	Sc "Conditioned" stimulus	Rc Response to C.S. (not necessarily the first)	Su Unconditioned stimulus	Ru Unconditioned response	Result	Influence on the bond between C.S. and the response given
Maze .	Placed in maze	Entrance to blind alley	Blank wall obstructing way	Retreat from alley	Less likely to enter alley	Weakened
Maze .	Placed in maze	Entrance to goal	Sight of food in goal	Increased entrance to goal	More likely to enter goal	Strengthened
Problem box..	Placed in box	Stepping on trigger	Sight of open door	Removal from trigger. Approach to door	More likely to step on trigger	Strengthened
Drive box..	Placed in box	Advance in direction of object (food, female, etc.)	Electric shock	More rapid advance toward object	Less likely to advance toward object when placed in box	Weakened

N.B. The bond under discussion is always that between Sc–Rc, the S in Col. 2 and the R in Col. 3. The underlining indicates that Rc and Ru are similar.

In Table I, I have given four 'set-ups.' In all four the stimulus of some supposedly dominant or unconditioned reflex follows the stimulus (and consequently the response) of some 'non-dominant' reflex. The first two instances support the conditioning explanation. In the third instance the conditioning formula is a duplicate of the first and yet the results are opposite. According to the conditioning explanation the unconditioned response (removal from trigger) should be attached to the conditioned stimulus. The opposite is the case. The more often the conditioned stimulus is followed by 'sight of open door—*withdrawal* from trigger' the more likely will it (the conditioned stimulus) be followed by *approach to trigger*.

In the fourth instance we have a set-up which, from the conditioning point of view, corresponds almost exactly with the second case. In the 'drive' box the rat reacts by advancing toward the food or sex object through an intervening corridor. Once in the corridor, a door is dropped behind him and an electric shock applied. This is the unconditioned stimulus for a more rapid and more definite advance. According to the conditioning hypothesis this should result in the strengthening of the first bond. The fact is, however, that on the next occurrence of Sc the animal is less likely to advance toward the food or sex object. Objection will be taken, of course, to the lack of similarity in the response in the fourth instance. Progression toward food is not the same as progression away from pain. But neither, in the case of the first instance, are the general investigatory movements which lead to entrance to the goal the same as the definite advance toward food, and yet this is the classical example of selection by conditioning.

Thorndike [5] in 1913, while discussing the congruity theory of Hobhouse and Holmes, pointed out that the accident of identical responses may go with the strengthening or weakening of the bond producing the first of these responses. Congruity or identity of response being features independent of the resulting condition, cannot explain a learning which is essen-

[5] E. L. Thorndike, Educational psychology. The original nature of man. 1913.

tially adaptive and any phenomenon to which the label of learning is applied implies adaptation.

III

Before discussing an alternative explanation it might be best to point out some of the assumptions upon which that explanation will be based.

I believe that during learning a potential more or less constant stimulating situation is assumed. To successive presentations of a constant element in the stimulating situation we have first a variety of responses and then one response following fairly regularly upon each presentation. Moreover there is implied the suggestion that the selected response is in some way objectively different from the responses which appeared once and were not repeated, and that difference must be in the direction of greater adaptability.

The above conditions are not presented as facts but as necessary implications of learning as it is commonly understood. Without a constant element in the stimulating condition there would be no need to explain the variety of responses. We would merely have one response to one situation and a different response to a different situation. We would then have the problem of explaining either a constant response to inconstant stimulating conditions or the establishment of a constant stimulating condition. None of the last three suggestions conforms to the ordinary idea of learning. The ordinary idea of learning implies, then, a constant stimulating condition.

Variety needs no defence. At least two responses are necessary before the phenomenon of learning would ordinarily be considered to have occurred. Similarly the necessity of a constancy of response greater than chance would bring about will not likely be disputed. There might be less agreement on the suggestion that the selected response must be marked off by some feature independent of the mere fact that it is selected. A phenomenon, however, consisting of a constant element in the stimulating condition followed at first by a variety of responses and later by a constant occurrence of

one response would not ordinarily be labelled learning. If in a maze a rat first went into alley A and went to sleep, and on the next trial went into alley B and slept and for ever after on subsequent trials went into alley C and slept, he would not be judged to have learned the maze. I believe most people would either hold that *no* learning had taken place *or that alley C must have a superior attraction for the rat.*

If we accept the position that in learning the selected response has greater survival value than the unselected responses, certain important theoretical and practical implications follow.

If learning is adaptive we must seek our selection principle in some factor definitely associated with utility. Now the utility of a bond is a product of the response it produces and the situation which follows that response or coexists with it. The selective or inhibiting agency, then, must be associated with that combination of response and subsequent situation. Later on the suggestion is made that this is accomplished by the retroflex principle. Since the inhibition of a bond means modification of an organism, and since both the response and the subsequent situation present stimuli, we have merely the case of a combination of stimuli (from both response and subsequent situation) affecting an organism (by altering a connection).[6] This point will probably bear some elaboration.

If the survival value of a response is an indeterminable matter until after it has occurred and if the valuable response (from the survival point of view) is selected more often than can be explained by pure chance, we must suppose some mechanism released by some factor correlated with survival value. Now if the survival value does not exist until after the occurrence of the response the selecting mechanism cannot be released until then. That is to say, if learning involves the selection of bonds leading to objectively determinable results it also involves retroaction.

This leaves us with two problems on hand. First, how can a mechanism be set off by such an abstraction as bio-

[6] Rizzolo in *Brit. J. Psychol.* (Med. Sec.), 1929, 9, 258–262, has found that peripheral stimuli do affect the chronaxie of the corresponding cortical motor area.

logical value? Second, how can a bond be influenced by conditions coming into existence after it has been activated? Perhaps these problems will receive some elucidation in the discussion of the practical implication of the principle of learning resulting in valuable bonds being selected. This practical implication lies not so much in the fact that the valuable bond is selected as that the selection is based on some criterion apart from selection itself. Grant some independent means of determining the response to be selected and learning reduces to a fairly simple formula.

Suppose that scattered about my room are lights of various sizes and that there is one switch for each of every possible combination of these lights. Suppose further that it is important that when I press a master button the intensity of light reflected from my desk be x candle-meters. What must I have? First I must have some mechanism which will do one thing when the intensity of the light is x and do something else when it is not at x. I use a selenium cell so arranged that a light of x will swing a needle to a certain point A and any other intensity will bring the needle to some other point and that when it is at any point other than A the needle will close a circuit through a motor. We have now only to arrange the motor so that when it runs it will keep throwing in one combination of lights and turning off others. When the light reaches intensity x the needle will return to A and the motor will cease changing the switches. So long as the intensity continues at x that particular combination of lamps will remain lighted and next time I press the button that combination will appear immediately. If however I move my desk, or the incoming current varies, then if the intensity changes from x the altering mechanism will operate once more and keep on operating until the light reaches x again or the repertoire of combinations has been exhausted. Thus with a mechanism capable of differentiating between two conditions, learning merely needs the addition of some mechanism which will keep on changing connections until one rather than the other of those conditions appears.

In connection with the possibility of influencing connec-

tions after the passage of the neural impulse, it will be noticed that the connections between the button on my desk and the lights are all altered after the current has begun to flow. They could just as effectively be altered after it had ceased to flow. We should not confuse the existence of a connection with its activation. The rat is trained to form a discrimination habit. The fact that he will do such and such in this situation implies a connection. This connection exists before it is manifested by the performance of the given response. It may just as probably exist afterwards. If it exists it may be affected. Lashley [7] has been able to disrupt such connections by operative technique. Before the rat went to the operating table there was a definite connection between being in the box and turning to the right. When he leaves the table there is no such connection. It has been affected after it has ceased to carry the neural impulse. If it can be affected by the artificial interference there is nothing unscientific in thinking of it as being affected by physiological methods.

Regarding the part played by values, we merely need to say that it is not values which set off the stamping-in or stamping-out mechanism but specific neural conditions. Some neural conditions stamp in some bonds and others stamp out some bonds. However, in the history of the race those neural conditions were associated with external conditions which had a biological significance.[8] If one organism was so arranged that its stamping-in mechanism was set off by conditions which later proved to be disastrous (*i.e.* if it persisted in producing disastrous conditions) for itself or for the race, either itself or the race would tend to vanish. The animal whose stamping-in mechanism was set off by conditions later proving valuable for itself or the race would tend to perpetuate mechanisms set off by similar conditions. Now if heredity were perfect and if the biological value of a con-

[7] K. S. Lashley, Studies of cerebral function in learning, II. The effect of long continued practice upon cerebral localization. *J. Comp. Psychol.*, 1921, 1, 453–468.

[8] Sandiford in "Contributions (of Thorndike) to the laws of learning," Teach. College Record, 1926, 27, 523–531, suggested that biological conditions might account for the origin of satisfaction and annoyance. Herbert Spencer had suggested this in "Principles of Psychology," 1871, 2, 280.

dition remained constant from generation to generation, we could say definitely that only biologically valuable connections would be stamped in. In so far as the neural condition of the parental stamping-in mechanism is not attached to the same mechanism in the offspring and in so far as a condition which resulted in survival then would result in destruction now (or vice-versa) or a new condition arises which never had an opportunity to incite either stamping-in or stamping-out tendencies in ancestors, to that degree we would expect our formula to fail. If we can assume that stimulating condition A retains its stamping-out influence from one generation to the other we can say that bonds are selected which resulted in conditions favorable to the survival of the race.

By stimulating condition in the above I mean the sum total of all the conditions which affect the nervous system immediately after the passage of a neural impulse. These come from many sources, among which we might mention kinæsthetic impulses, impulses from resultant external stimuli and those from organic conditions.

The first two of these may be thought of as being produced by the conditions in columns 2 and 3 of Table I.

If we can assume that there will be an effective difference between the sum total of sense data produced by 'Entrance + Contact with wall' and that produced by 'Entrance + Contact with food,' we have our fundamental condition of learning. As in the case of our automatic light, all we now need is some mechanism which will be set off by the first set of sense data and not by the second, and will stamp out the bond which has just been the vehicle of an impulse. We may if we wish suppose a second mechanism which, while unaffected by the first set, will be set off by the second set and will tend to stamp in such a bond. This is not necessary, however, as if all 'wrong' bonds are stamped out the correct one must necessarily be retained.

Thus far the position here taken is largely in harmony with the point of view I briefly outlined in a previous article [9]

[9] J. M. Stephens, A mechanical explanation of the law of effect, *Amer. J. Psychol.*, 1929, 41, 422–431.

and also similar to Troland's [10] use of the principle of retroflex. I believe a slight modification of both those positions lies in the insistence on the importance of the combination of response and subsequent situation. Troland's nocioceptive stimulus is almost identical with a pain stimulus. I do not believe that any external stimulus consistently works in one direction regardless of the previous response. This may be cleared up by some more examples of learning. Let us suppose that we have six animals which we submit separately to the same stimulation (Noise at A). Suppose further that in the first response three animals approach A and the other three run from A. Suppose that we consistently provide subsequent stimulation as follows:

TABLE II

Animal	Stimulus	Response	Subsequent Stimulus	Nature of S.S.
1	Noise at A	Approach to A	Food appears at A	Beneceptive
2	" " "	" " "	Nothing definite at A	Neuteroceptive
3	" " "	" " "	Ferocious animal at A	Nocioceptive
4	" " "	Flight from A	Food appears at A	Beneceptive
5	" " "	" " "	Nothing definite at A	Neuteroceptive
6	" " "	" " "	Ferocious animal at A	Nocioceptive

We would suggest that if we continued the treatment to each animal, animals 1 and 6 would be likely to retain their first responses to the stimulus and the others would be less likely to do so. Yet in the case of animal 6 the bond's operation is followed by a nocioceptive stimulus—the same stimulus which in combination with approach tended to stamp *out* the bond which had just operated. With animal 4 a beneceptive stimulus stamped out the response. We see again that it is the total stimulating condition produced by the combination of response and the external stimuli which must be regarded as the effective selective agent.

Moreover this combination is enough to account for learning. Given the response and the subsequent stimulating conditions we can predict whether the bond will be more or

[10] L. T. Troland, Fundamentals of human motivation, New York, Van Nostrand. 1928.

less likely to operate again. Thus in Table I we can dispense entirely with column 5 (unconditioned response). The selective agency is sufficiently given in columns 3 and 4.

The action of this agency could, of course, be specific. Each bond could have any number of specific sum totals of sense data which would stamp it in and others which would stamp it out, and others which would leave it unaffected. Other bonds could be affected in a totally different way.

If, however, the relation between bonds and specific groups of sense data is a racial or inherited arrangement, we would expect that certain of these would work in definite directions as indicated in Table II.

While the law of effect does imply connections between certain groups of stimulating conditions and certain responses, it does not imply that these are necessarily synaptical.[11] The mechanism of retroflex action can be made to produce learning where the potential connections are between one sending and several receiving radio stations or tuning forks, or neural frequencies. The schema of '*stimulus response subsequent stimulus combining with the response to affect the connection*' remains.

This is not presented as a complete formulation of learning, but as an attempt to consider some of the essential features of any explanation. The lack of definiteness in the idea of 'a combination of response and subsequent stimuli (really a summation of subsequent sense data) associated with a situation which tended to increase (or decrease) the probability of individual or race survival,' is admitted.

The law of use is ignored in this account. It is also ignored in other sciences in all of which it plays as much part as in the determination of learning. If a force acts on an object then two applications of that force will have more influence than one application. If a total situation is such as to release a stamping-in force then the more often that situation occurs, the more stamping-in will result. If on the other hand, a stamping-out force is released, the law of use

[11] I find the synapse a very useful concept. The theory of retroflex is, however, not dependent upon that concept.

works the wrong way.[12] There is, of course, an important difference. The law of use was formulated with an eye to something different than connections subject to causation. In other words, the injunction 'Repeat the functioning of a bond' did not mean just that. The whole problem of learning is concerned with trying to find out how we can arrange for the repetition or non-repetition of a bond. Taken at its face value the law of use assumed that we knew how to do the thing which it was telling us how to do. That injunction really meant 'subject the organism to the situation which produced the evidence of the existence of the bond.' Whether the bond will function again or not depends on the direction in which the forces of learning have been acting.

In view of the above it might be interesting to inquire into the reason for the vogue of the law of use. Three reasons suggest themselves. First, repetition of a bond is only possible when stamping-in mechanisms are at work. If a bond is stamped out it will not be repeated. Consequently repetition was only possible with stamping-in—was always associated with stamping-in, *but with the causal rôles the reverse of the popular conception—with stamping-in the cause of frequency*. Second, in obeying the only part of the injunction under our control (*i.e.* repeatedly subjecting the organism to the same stimuli) we provided an opportunity for stamping-in or -out forces to occur, and the law of effect would tend to produce the desired (*i.e.* valuable) bond. Third, bonds were considered as very gross affairs. Skating was a bond. Here we do not consider skating as one act which one gradually comes to perform better, but as a multitude of bonds some of which are eliminated and others retained. In accordance with the suggestion above, those eliminated would be the less valuable and so we would come to skate better. Thus anyone thinking of skating as a bond would naturally ascribe its 'improvement' to use. To one thinking of learning as the overcoming of original bonds, consistent repetition of those bonds is the negation of learning.

[12] K. Dunlap, A revision of the fundamental laws of habit formation, *Science*, 1928, 67, 360–362.

There remains the task of linking conditioning to the principle of retroflex and to account for the fact that conditioning has often been considered as something apart from trial and error learning. I believe that the chief reason lies in the identity of responses in conditioning. It is rather surprising to see the original response of one stimulus become the acquired response of another stimulus. Yet when one stops to examine the raison d'être of the original or dominant bond, the phenomenon of identity of responses is rather to be expected (from our general schema of learning) when certain incidental (in the sense that *learning* could occur without them) conditions are fulfilled.

In the first place many of the 'dominant' bonds or tropisms investigated have been found capable of modification. If a bond is susceptible to modification and has not been modified, it must have been subject to stamping-in more than stamping-out conditions. This means that the response which consistently formed the end point of that bond must have combined consistently with the subsequent situation to produce a valuable condition.

In other words, that response to that stimulus has proved valuable. Now in many cases such as food taking, the value of 'approach (or salivation) followed by food' is just as valuable as 'food followed by approach (or salivation).' Hence any bond producing the former will just as probably be stamped-in as a bond resulting in the latter. The fact that food is regularly followed by approach shows the value of the combination. In this case the value is not impaired if the order of the stimulus and response is reversed. In the problem box the value is impaired by reversal.

To an animal entering the ordinary unhealthful situation there are two possible protections. He will have a better chance to survive if (*a*) he leaves there and if (*b*) he does not again return. That is, if the animal is to survive, the unhealthful situation should (*a*) produce a new response and (*b*) should stamp out the bond which resulted in the approach. We may assume with the law of retroflex that both of these things are accomplished by the same situation or (with the

conditioning hypothesis), that the situation causes the withdrawal and that the accompanying neural impulse stamps out the previously operating bond. The crucial point seems to be that in the case of the problem-box type of learning where it is unhealthful (relatively) to be on the trigger (after the trap has sprung) but not unhealthful to go there again, the withdrawal reaction does not stamp out the approach reaction. The response selected (the producing bond not being stamped out) by a subsequent situation and the response initiated by that situation are the same when only one type of response produces a valuable condition in connection with that situation, whether the response precedes or succeeds the situation. In the case of some situations this is true. To the typical unequivocal food situation there is only one valuable response for an unequivocally hungry animal. That response is approach. Similarly, if a response is to be followed by a food situation, only an approach response will be valuable. So that if any situation habitually precedes a food situation it will produce an approach response,—not because food also produces an approach response, but because food following such a response produces a stamping-in condition for the causing bond and a stamping-out condition for a bond leading to any other response. In the case of situations as in the problem-box, where the response which the situation produces would not be valuable if occurring before the situation, the response selected and that initiated by the situation are not the same.

Summary

The underlying idea of conditioning (the influence of one operating nerve tract upon another operating tract) is inconsistent with the actual conditions under which it is produced. The latter fit more readily into the trial and error schema.

The selection of bonds cannot be explained as conditioning, since selection can take place contrary to the conditioning formula (problem-box) and the formula can be applied with no resulting selection (drive box). The description of conditioning in the simple terms of dominance and time order is

deceptive because dominance is often a product of learning—the phenomenon which conditioning attempts to explain.

The retroflex or mechanistic law of effect is presented as an alternative. In this it is assumed that surviving organisms must have been able to select survival-favoring bonds. The survival value of a bond is a product of its response and the conditions subsequent to the response. These are held to affect the bond (*i.e.* the organism) by the combination of their respective stimuli. The essential elements of the conception have been synthesized into a machine which modifies its own responses to achieve a given end. The hypothesis is held to cover any case of learning. It presents a schema which facilitates a direct frontal attack on the problem of definiteness. If the selective agency is in the combination of response and subsequent situation, we may proceed to find out which combinations (in terms of objective attributes) stamp in and which stamp out. Once having done that, we may perhaps achieve a generalization *stated* independently of survival value.

In the retroflex principle the law of frequency cannot be considered as a causal agency.

The identity of responses in conditioning is explained by the fact that in certain cases the nature of the subsequent stimulus is such that only one response (whether preceding or following) will form a valuable combination with it.

The 'explanation' throughout is limiting rather than inclusive. It is not suggested that all bonds are affected by all sense data subsequent to their operation, but that when they are affected they are influenced in the directions indicated.

[MS. received October 22, 1930]

EFFECT AND AFFECT IN LEARNING

BY H. L. HOLLINGWORTH

Columbia University

The outstanding mystery in the psychology of learning is of course the so-called 'law of effect.' Part of the mystery arises from that ambiguity of the law which leads to confusion between 'effect' and 'affect.' As often stated, and as expressed in Bain's early formula, it is *affects*—agreeableness and disagreeableness, pleasure and pain—that are responsible for the result. Pleasure reinforces the act that led to it; annoyance tends to eliminate the act producing it. On this basis the law should be called the 'law of affect.'

More modern statements of the law, in terms of satisfaction and annoyance, easily lend themselves to this interpretation. Agreeableness, it is often said, stamps in, and disagreeableness stamps out, the connections or movements leading to them. It is the *feeling tone* that does the trick; the *affect* that is effective. The affect is to be sure conceived as the *effect* of the act, its consequence or result. But the virtue is attributed, in such statements, to the affection, not to its status as effect or consequent.

As we have intimated, however, another form of the statement is possible, in which it is the *effect* or result, rather than the *affect* or feeling-tone that accomplishes the trick. This is in fact the form that Thorndike gives the law in his more guarded statements; it is the 'state of affairs' resulting in satisfaction, not the satisfaction itself, that is given the credit.

According to what we may then call the law of *effect* (as distinguished from affect), it is the *result* of an act, its consequence, what it leads to or accomplishes, that determines the fate of the act. Such a statement slurs the subjective fact of feeling-tone and stresses instead the more or less objectively produced change.

The difference is important. The law of *affect* describes something within the subjective experience of the actor alone. The law of effect at least does not emphasize this feature, and it may even describe some objectively verifiable change in nature instead. The ambiguity is perhaps excusable because the facts are complex. Thus it is perhaps not the bare occurrence of the objective change, but this change as related to the wishes and motives of the actor that may be supposed to be effective. And since feeling tone also often depends on this relation of objective occurrence to subjective wish, there is likely to be some coincidence between effect and affect. It may even be those effects correlated with the pleasure-affect that act one way, as opposed to those effects correlated with the annoyance-affect that act the other way.

If this correlation were always present, then the only problem would be whether it is the effect, as objective fact, or the affect as subjective experience, that does the magic. Even if the former were always the case, it might be convenient to use the correlated affect as the sign of the presence of the actually operative effect. Those acts would be established whose effects were accompanied by or correlated with the affect of pleasantness. There might even be some systematic warrant found for considering effect and affect as merely two aspects of some single real fact.

However, in the discussion of the topic, these subtleties have not usually been adopted. On the one hand it has been explicitly affirmed or denied that it is the feeling tone, or its physiological correlate, that has the virtue. And on the other hand, when experiments have been performed, the manifest 'satisfaction' or 'annoyance' has by no means always been intrinsically related to the act. It has instead often been irrelevantly introduced, as in the arbitrary bestowal of rewards and choice tid-bits, or artificial punishment of some kind.

We can advance a few steps in this topic if we first consider carefully the systematic psychology of motivation. It is in this connection that such terms as 'satisfaction' and 'annoyance' originate. The conventional accounts of man's

original constitution place satisfaction and annoyance on a par, as equally primary facts of human nature. The inventories begin with the indication of various 'original satisfiers' and 'original annoyers.' Now this is much like describing the sky by noting that 'to begin with' there are certain clear patches and certain cloudy areas. If you then treat the clear patches and the clouds as equally primary, describing their respective behaviors, you may make some progress, although in every instance your descriptions would be needlessly duplicated.

Something is to be said for first discovering whether clouds or clear areas are the positive, active elements; or at least for assuming one as primary and letting this assumption govern the description until you see what it leads to. Thus you might describe the sky by portraying the movements and whereabouts of the clear patches. But after all it is not the clear patches that *do* anything; it is the clouds that rain. The clear or cloudless areas are merely regions where there is no vapor. Clouds are the positive elements; thence come thunder, lightening, hail and rain. Clear areas are in a sense only negative facts; they are unoccupied regions. Of course they may be none the less beautiful or conspicuous because of this fact.

Applying the analogy to the psychology of motivation, annoyances are the clouds and satisfactions are the unoccupied stretches. Pleasantness is the neutral background; disagreeableness and irritation are the positive objects that from time to time accumulate, condense and precipitate into action of the sort that leads to learning. Pleasantness is of course not mere sleep or stupor; it is activity also, but activity of the background sort, instigated by transient cues as distinguished from motives.

We can, then, start the description of human nature in one of several ways. We can begin by putting satisfaction and annoyance on a par, as equally positive, effective facts. This has been the usual method.

Or we can start with 'pleasure giving' trends as the elementary material. Annoyance will then represent only

interference with or blocking of these positive conative pleasure drives. This procedure has been advocated and attempted, most recently and systematically in the psychologies of Stout and McDougall; more anciently in the philosophical and ethical systems of 'hedonism.'

I believe both these methods to be misconceived. If they do not put the cart before the horse, they may be said at least to exalt the inert clear areas over the active cloud formations.

There is but one other way—that of considering the irritant, the annoyance, as the primary fact of motivation. This seems to describe the facts correctly. To begin with, a stimulus is by definition a disturbance. It is something which occurs to upset the equilibrium of a system. And all that a stimulus primarily does is to work itself out—that is, it eliminates itself. The stimuli to men's active endeavors are always irritants, itches, aches, pains, distresses, cramps and tensions—the so-called 'disagreeables.' They are the prime movers to action. The acts to which they lead are either by chance, or by original constitution, or by learning, such acts as destroy the irritants which provoked them. The *satisfaction* of such a motive consists not in some added positive fact, but merely in the *removal* of that motive. Irritants, annoyances, are the positive clouds; with their precipitation and disappearance, pleasantness intervenes.

Let us take the classical case of the fly on the infant's cheek. In the absence of appropriate inherited nervous connections, this event is a vague and diffusely effective irritant. Its disturbance flows out in this and that direction. Until something happens that dislodges the fly, its presence is a persisting irritant, that is a motive, a source of prolonged activity and restlessness. If a certain sweep of the hand occurs, the fly is swept away. The irritating stimulus or motive being gone, activity occasioned by it now ceases. With repetition we note that this effective technique of alleviation tends to come more promptly, until in time it is a well-established adjustment. It has been learned.

Now what has happened? Presumably the law of effect has operated. But what was the effect? The effect pro-

duced was obviously the removal of the irritant. Apparently we may generalize the instance by stating broadly that irritants become linked to the activities which eliminate them. Of course satisfyingness also occurs, but this is a mere incident; it is the negative aspect; it is the clear background that reappears when the clouds roll by. Now why does the irritant become linked with the act that annuls it, rather than with any of the various 'unsuccessful' acts?

The 'successful' act, of course, completely drains the energy of the irritant. All its energy, to the last fraction, is worked off over this route, since when the act is achieved the irritant is itself abolished. It would be tempting to stop with this statement that stimuli become linked to acts which completely discharge them, thus making completeness of discharge the potent fact in learning. But in this sense the stimulus has always been completely discharging. Every 'random' action pattern evoked from moment to moment may be said to have drained off all the energy at that instant provided by the irritant. And yet these random and unsuccessful movements and action patterns are not the ones ultimately established.

There is however a sense in which 'completeness of response' is suggestive. The point is that this 'successful' act *terminates* the situation or series of which the fly was the initial step. This is its *effect*. *That* series is now over. What happens next will be due to other motives, to irritants elsewhere in the system. Each series, from the incidence of its irritant to its removal, represents a definite unit or segment of activity. Considering a single series as such a unit, the *terminal act* is the *response*. All that precedes this terminal act constitutes its ground, its antecedent, its temporally and otherwise patterned stimulus or occasion.

The one fundamental law of learning is the following: *Learning occurs in so far as partial antecedents become effective in evoking the terminal act.*

All learning is cue reduction. In a general way, *any detail* of an antecedent complexity or series may, in a protoplasmic system, tend to lead directly to the consequent formerly resulting from more complex antecedents of which

this cue detail was once a part. The various special laws, concerned with such factors as frequency, context, recency, and the like, are those which experimental psychology has studied in considerable detail, though often with obscure understanding.

Given such a general tendency to cue reduction as characteristic of protoplasmic systems, it is clear that special potency will soon come to attach to any particular detail or cue which *always* occurs in the series prior to the terminal act. Such a detail, and the clearest case of an omnipresent detail, is precisely the *initial irritant.* Without this the series never happens at all. Given this, there is always likely to be an activity series, closing with the terminal act which annuls the irritant.

It thus seems inevitable that an irritant will ultimately become linked with its terminal relief act. Given the irritant, the terminal act will be promptly instigated. Intermediate steps will tend to be eliminated. For in so far as any one does occur, it also will tend strongly to evoke the terminal consequent. We might even expect, in some cases at least, that the later in the series the 'random' act first occurred, the earlier would it be eliminated in the course of learning. Earlier steps would, if they occurred, cumulatively make for the terminal act, toward which the initial stimulus was also leading. Later steps would be omitted if only for the simple reason that they have no time to occur.

Learning *is* then, in a sense, due to a law of effect (but not to a law of affect). Irritants, as stimuli, become linked to acts the effect of which is to remove them. Objectively considered, irritants are stimuli which persist until some act is produced which exterminates them. The subjective correlate of this activity is annoyance. Pleasantness is relief from irritants. The satisfaction of a motive consists merely in its removal.

Learning is due neither to the pleasantness nor to the disagreeableness, although either of these may be used as the sign of what is going on. Unpleasantness signifies a vital series in progress, which will continue until a terminal act is hit upon. Pleasantness (the disappearance of the cloud)

indicates that some terminal condition has occurred. But the learning is due to neither of these convenient signals. The learning is due to, or is constituted by, cue reduction, which is a fundamental protoplasmic phenomenon. Through the general law of cue reduction (redintegration) an initial irritant becomes potent to instigate directly the terminal act of the series it has formerly inaugurated.

An adequate psychology of learning involves the following features, if a complete account is to be given:

a. The general redintegrative law (which merely describes a fundamental feature of the behavior of protoplasm) and the special laws relating to frequency, recency, and combination of stimuli.

b. The distinction between transient cues (casual stimuli) and enduring irritants (motives). The former come and go without particular reference to the behavior of the system. The latter persist longer, often until alleviated by some adjustment of the system.

c. Varying degrees of scope (sagacity) which permit varying degrees of comprehension or joint-action of several different cues available in a given situation as objectively described.

d. The concept of an activity series, with its initial irritant, its intermediate steps, and its final terminal act which eliminates the irritant.

We are not here concerned with all these features, which have been considered in detail elsewhere. Our present concern has been solely with the famous 'law of effect.' We have attempted to give it a clear and systematic formulation, adequate to the facts of observation, and yet free from all 'hocus pocus.' Only the simplest sort of learning situation has been referred to. But more complex acts of learning seem to involve no additional requirements, once they are correctly analyzed and their course of progress correctly described.

[MS. received November 1, 1930]

HUMANIZING THE APE

BY W. N. KELLOGG

Indiana University

The opinion seems to persist among certain contemporary psychologists that a sharp qualitative demarkation between the behavior of man on the one hand and the behavior of infra-humans including the anthropoid apes, on the other hand, is an established fact. The strength of this view is evidenced in part by the recent controversy involving the doctrine of instinct and the support which this conception continues to receive from some quarters, especially with reference to the interpretation of the activity of animals. It is the object of this paper (*a*) to point out in this connection an aspect of comparative work with higher primates which appears to have been thus far overlooked, and (*b*) to propose a technique of investigation which will take account of this new factor. Our general thesis is that despite the evidence which indicates that the anthropoid apes are inferior to man in behavioral potentialities, it is quite possible that a radical change in experimental procedure would definitely remove many of those qualitative distinctions that are frequently thought to exist.

1. The Effect of Environment

Without recourse to further introductory discussion, we may point the way to the initial arguments to be considered by the following question: What would be the outcome if a human infant, the child of civilized parents, were placed in the environment of the jungle or in some similar situation, and allowed to mature in these surroundings, without language, without clothes, and without the association of other humans? Fortunately it is not necessary to rely entirely upon speculation to answer this question, for a number of cases are on record of the discovery of 'wild' children, who have been reared from an early age with little or no human

contacts.[1] Although not all such instances are perfectly authenticated, the facts in some of them are established beyond a reasonable doubt.

One of the earliest of these children to attract scientific notice was 'Itard's wild boy' who was found in 1798 by a group of French sportsmen (**13**, p. 137). The child seemed to be fully ten years old, but he was unable to talk and had been living, so far as could be ascertained, on whatever provender he could find in the forest. He was taken to Paris and after a long period of relatively ineffectual training was pronounced mentally deficient.

The Kasper Hauser case, another notable example, is doubly important because there is no question of its authenticity. This boy, who has been variously regarded as a royal pretender or as an heir to some princely German house, was apparently put out of the way by political schemers of his time. He was confined alone in a dark cell so small that he could not stand upright till he was 17 years old and was fed on bread and water throughout this period. No one saw him except his keeper. When found in 1828 he could walk only with the greatest difficulty and scarcely knew how to use his hands and fingers. He could not understand what was said to him, was able to speak only one sentence, and was ignorant of the most elementary facts of everyday life. He possessed, however, a remarkably keen sense of smell and a capacity to see in the dark far surpassing that of the average person. Intensive educational training was only partially successful because, according to Tredgold (**19**, p. 301) "the prolonged isolation had wrought an effect upon the brain cells from which they could not completely recover."

Of a number of more recent instances, the 'wolf children' of India are probably the most striking. Two of these children, one of whom is presumably still living, were found as recently as 1921 in a cave inhabited by wolves (**16**). Their ages were estimated at two and eight years respectively. When discovered, they had no language responses and could

[1] According to Murphy (**13**, p. 137) about a dozen such foundlings are known to history.

not walk upright, but instead crawled about on all fours. They ate and drank like dogs, making little or no use of their hands in these activities. The younger of the two died sometime after removal from the cave and the other, a girl, was kept in the household of a Christian missionary, who named her Kamala and who undertook with the assistance of his wife to provide her with a special course of education. At the completion of four years of this training Kamala could speak no more than 40 words and still uttered strange animal-like howls at night. Efforts to break her of the habit of pouncing upon and devouring small birds and mammals had not been successful. Although the child eventually learned to walk, she is reported never to have learned to run.

Instead of supposing that these children were congenitally feeble-minded as has usually been done, I submit that originally they probably possessed an entirely normal equipment of reactions—otherwise survival against the terrific environmental influences would have been impossible. On the strength of this view, it would seem that they had made natural and adequate adjustments to their surroundings. They seem, in fact, to have developed responses which were particularly suited to their immediate contacts. Those placed with wild animals *learned* themselves to be wild animals in a literal sense of the word. When suddenly transplanted, therefore, to a highly organized society which was entirely foreign to them, they had no adequate responses available and were as a result stigmatized as feeble-minded.

Their inability to acquire the desired kind of behavior even with careful training is assignable to the fact that they had advanced to too mature an age to uproot the fundamental habits so basically entrenched by earlier experience. This explanation follows readily from the recognized importance of the very early years in psychological development. Watson, Kantor, and others have held, in fact, that the baby at birth represents virgin soil which can be cultivated by special training in any direction. Criminals and geniuses are made, therefore, by a genetic process, rather than born. One does

not necessarily need, on the strength of this assumption, to conclude that all feeble-minded children, like criminals and geniuses, are feeble-minded as a result of deficiencies in training. Certainly congenital defects prohibit normal development in a great number of cases. With the perfection of efficient methods in clinical psychology, however, a large percentage of children previously diagnosed as feeble-minded have been proven to be sound in all respects except in equipment of acquired reactions. If discovered at an early enough age the 'inherited' deficiencies of these individuals have been satisfactorily corrected through specialized education, although this has not been possible if they have persisted too long in their original habits. 'Wild' children, according to this view, should be regarded as feeble-minded only to the extent that higher animals raised under like conditions might be expected so to be.

2. Experimental Support

Even if one accepts, however, the necessarily great influence of genetic factors, does not the mass of experimental data from animal psychology definitely *prove* the ape inferior to humans? Much of the available evidence appears, indeed, to have been interpreted in this direction. Yerkes, however, who has probably done more work with anthropoids than any other contemporary psychologist, has recently remarked (**31**, p. 191): "It is indicated by current research . . . that behavioral adaptivity is qualitatively similar in man and in anthropoid ape. . . ." Some few comparisons in which the environmental preparation of the humans has been no different from that of animal subjects (anthropoid or otherwise) point strongly as well to a similarity rather than a difference in the abilities studied. Thus Alpert (**1**) has shown that young children behave much like Köhler's (**10**) chimpanzees in solving similar problems. Hicks and Carr (**7**) have demonstrated that man possesses little if any superiority over rats in learning to run a maze; Warden and Baar (**22**) have found that birds as well as humans are subject to the Müller-Lyer illusion; and Gesell (**6**, p. 344 *ff.*), who tested an

adult street monkey of the species *Cebus sapajous* with his standardized tests for infants, discovered many similarities to the behavior of young children. Witmer (**24**) in fact, at one time examined a trained theatrical chimpanzee, Peter, in the Psychological Clinic of the University of Pennsylvania. The ape was not only able to perform many simple tests commonly employed with children, but even went so far as to imitate the tester in writing the letter 'W' on the blackboard—a problem which so far as is known was in every aspect new to the animal. This anthropoid, whose age was probably not more than five or six years, was considered by Witmer to be equivalent on a human scale to a low or middle grade imbecile.

Evidence is indeed not entirely lacking to show that it is possible for primates under certain conditions to perform at least one elementary psychological test more proficiently than humans. A recent study by Tinklepaugh (**18**) who worked with one *Macacus cynomologus* and three *Macacus rhesus* monkeys in a modification of the delayed reaction experiment, brought out the surprising fact that these animals were frequently superior to the experimenter and to various observers who entered the laboratory (p. 205). Comparative studies with two twin boys four years and nine months of age showed furthermore that the children exhibited behavior strikingly like that of the monkeys in similar problem situations.

3. Objections to Previous Investigations

There are two serious criticisms which may be brought to bear, it seems to me, against nearly all experiments upon the behavior of apes which have thus far been attempted:

1. Since these have been chiefly of the analytical type, *e.g.* concerned with the study of special abilities such as sensory discrimination, memory, insight, learning, *etc.*, they have necessarily been undertaken with animals at least sufficiently mature to be independent of maternal care. It is doubtful, in fact, if anthropoids have ever been employed for genuine experimental purposes at ages much under two or three

years. Hence the most formative period in these animals' lives has been spent with others of their kind in the acquisition of typically infra-human modes of response. How could they ever be expected under these conditions to develop basic reactions which were not predominantly of an animal nature?

2. A much more significant and generally overlooked point, however, is that without exception the laboratory animal is treated by the experimenter, and by all others with whom he comes in contact, essentially as an animal. Although elaborate precautions are taken to eliminate 'secondary cues' during the performance of the tests, *primary cues* of the most disturbing sort are entirely overlooked outside the test periods. The animal is never given a chance to learn human behavior. Everything is against him from the start. He is kept in a cage, in a characteristically animal environment, or he is led about on a chain or leash. He is fed like an animal, must sleep like an animal, and is gaped at and teased by curious bystanders. He may even be poked with a stick to 'see what he will do.'

Under the most favorable circumstances he is seldom used for experimental purposes for more than two hours a day. At least seven-eighths of his waking life, therefore, is consumed in typically infra-human surroundings. Yet all our psychological conclusions regarding his behavior are based upon the short interim of experimentation and fail utterly to take this longer period into account. If the animal can learn laboratory tricks in one-eighth of his time, must he not learn a very great deal in the other seven-eighths even though no specific effort is made to motivate him by hunger or punishment? A conservative inference would be that he is not only permitted to continue in his animal ways by such a procedure, but that he is forced by *environmental circumstances* to remain upon the animal level. Could one honestly anticipate anything different from normal human children reared under like conditions, experimented upon in the same manner, and similarly caged when not being tested?

Even those splendid experiments which have done more than any others to bring out the highest types of behavior in

the highest types of animals, *e.g.* Hobhouse's (**8**, p. 235 *ff.*) and Köhler's (**10**) studies of chimpanzees and Yerkes' work with chimpanzees, orang-utan, and gorilla (**25**, **27**, **28**, **29**, **30**) are all subject to these specific criticisms. Considered in the light of their own limiting conditions, their results represent outstanding contributions to the investigation of anthropoid behavior, but as studies of the comparative psychology of humans and animals—and as such they are frequently interpreted—they seem to have ignored completely factors of vital importance. Indeed, the objections of the *Gestalt* psychologists to earlier experiments with animals (**9**, p. 167 *ff.*) may with some stretching be turned against their own work. Thus it may be said that although Köhler took account of the configural responses of the apes to his experimental situations, his findings are invalid for comparative purposes since he failed to consider the larger *Gestalt* of which the experiments themselves were only a minor figure.

By analogous arguments it can be shown that the anthropoids of Kohts (**11**) and Cunningham (**3**), which were primarily household pets, could never have risen above typical pet behavior, since the responses integrated by the environment and by the reactions of humans with whom they came in contact were essentially 'pet' responses.

4. The Genetic Method of Approach

The animal as well as the human must be definitely regarded as a product of its surroundings. There is no justification for ascribing to either a special immunity from environmental influences. Such meager evidence as we have, moreover, points decidedly to the fact that if the environment of an animal is changed sufficiently, *and changed at an early enough age*, entirely different behavioral characteristics will result. Thus Scott (**15**) and Conradi (**2**) have demonstrated that birds of various species reared with those of other species develop—not the song peculiar to their own kind—but a song like that of the foreign birds with whom they have been reared. The teaching of growing canaries to sing popular melodies has, in fact, since these classical experiments become

a well-established business (**12**). The young are kept from all musical sounds except those of the tune to be acquired, which is played to them several times a day—usually upon a phonograph. Most birds readily develop faithful reproductions of at least one melody which thereafter becomes their characteristic song.[1a]

An example of similar type is afforded in the case of the pedigreed German police dog, Fellow, whose master made a point of treating him more like a human than an animal from earliest puppyhood. According to statements of Mr. Jacob Herbert of Detroit, owner of the dog,[2] Fellow was constantly with human companions who verbally directed, instructed, and encouraged him. Never was he whipped. His environment so far as was consistent with social propriety was that of a child instead of a dog.

Fellow was able to understand, as a result of this development, an astoundingly large number of words and to respond to them in such a way as to remove all doubt of his thorough comprehension. He appeared before the Galton Society and the American Psychological Association and was examined many times by competent animal psychologists. Several detailed reports of his activities have been published by Warden and Warner (**20, 21, 23**). According to the view set forth in this paper, the dog himself is not to be regarded as inherently exceptional. It is his 'education' which has been so.

Here then is an animal, lower in the phyletic scale than the anthropoid, which displays surprisingly human characteristics as an outgrowth of its near-human environment and treat-

[1a] Z. Y. Kuo has very recently reported a study (The genesis of the cat's responses to the rat, *J. Comp. Psychol.*, 1930, 11, 1–35) in which he reared several groups of cats in different environments, some having rats as cage companions while others had only other cats. Of those which lived with the rats none ever killed a rat of the same species with which it was raised, although ample opportunity was afforded. On the other hand, the members of a group which lived only with cats but which were permitted to see their mothers kill rats, all themselves became rat-killers by the time they attained the age of four months. This author holds that if there exists therefore an 'instinct' in cats to kill rats, there must also be an 'instinct' to love them.

[2] Mr. Herbert is neither a psychologist nor a biologist, but a layman—and a great lover of dogs.

ment. The apes, consequently, which are morphologically closest to the human species, should be capable of much more striking development. Their long period of infancy and their length of life are similar to those of man; their hands permit them to perform many human tasks; their nervous system is markedly superior to those of birds or dogs.

5. A Proposed Experiment

Suppose an anthropoid were taken into a typical human family at the day of birth and reared as a child. Suppose he were fed upon a bottle, clothed, washed, bathed, fondled, and given a characteristically human environment; that he were spoken to like the human infant from the moment of parturition; that he had an adopted human mother and an adopted human father. Suppose further that he were placed in a baby carriage and wheeled; that he were given selected playmates—young children who would be reared with him—who could be counted on to treat him as an equal and not as an inferior or as an animal; that he were taught to walk on his hind legs as the human child is taught; and similarly that his education and his environment were modified, as he grew, in accordance with the standards of human society.[3]

Under no circumstances should the subject of such an experiment be locked in a cage or led about on a leash. Under no circumstances should he be fed from a plate upon the floor. The criterion for his treatment should be without exception the same as that of a human. Throughout his upbringing his mistakes should be carefully and persistently corrected as are the mistakes of a child.

The experimental situation *par excellence* should indeed be attained if this technique were refined one step farther by adopting such a baby ape into a human family with one child of approximately the ape's age. Genetic case studies of the two individuals could then be undertaken, supplemented

[3] The general plan for an experiment of this type is by no means original. As far back as 1909 Lightner Witmer wrote (24, p. 205) "I venture to predict that within a few years chimpanzees will be taken early in life and subjected for purposes of scientific investigation to a course of procedure more closely resembling that which is accorded the human child."

by such comparative tests as it seemed feasible to make throughout their development.[4]

Possible results to be achieved from this type of investigation can only be imagined. Theatrical apes have been taught remarkable performances. We know already that many of them eat like humans, dress and undress themselves, ride bicycles, skate on roller skates, smoke cigarettes (apparently with a relish) and understand a large number of human words. But these activities are learned as mere stunts which are performed only at stated intervals under very special conditions. They are not made integral and necessary parts of the apes' lives. The theatrical animals furthermore, like the experimental ones, are kept in cages or chained much of the time. Their respective environments, again, are predominantly those of an animal world.

6. Would Human Speech Develop?

Although the majority of investigators seem to regard human speech as quite beyond the capacity of the anthropoid ape,[5] it is to me not entirely inconceivable that under the genetic process outlined, systematized language responses—at least in rudimentary form—would be found to develop. Observations of chimpanzees have shown that they possess without special training a fairly well organized 'emotional language' and that they employ in many cases sounds which appear to be specific to particular behavioral situations.[6] It has long been known furthermore that the anatomy of the vocal mechanism of the higher apes is enough like that of man to permit the possibility of human speech. In fact, the reports of careful investigators indicate that the cry of the newly born orang-utan or chimpanzee is hardly distinguishable from that of the human infant.

If then during the process of uttering some infantile wail the ape baby in the human environment happened to close its lips, it would, quite by accident, pronounce the word 'ma.'

[4] It would probably not be practicable, however, to continue such an experiment after the organisms had reached the age of five or six years.

[5] *Cf.* the many references to this topic in Yerkes (26), Chaps. 13 and 24.

[6] *Cf. e.g.* Yerkes (32).

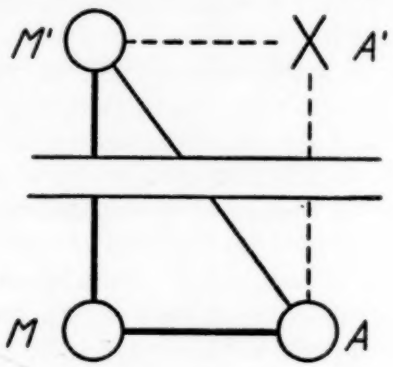
Its adopted parents should, however, at this point markedly *increase the stimulation* as is done in the case of a human child. Their activity would be characterized by picking up the infant, hugging and fondling it, repeating the word 'ma' to it many times with perhaps additional exclamations of 'Baby's first word!' Ultimately this method, which is almost universally practiced with the young of the human species, might lead in the ape as it does in man to the voluntary use of the sound 'ma' to indicate the female person who acts in that capacity. By various extensions of this procedure the word 'da' and the names of simple objects could next be learned. The same painstaking effort and corrections which are directed toward the acquisition of language responses in the child would, of course, be necessary in the case of the anthropoid.

Unless the ape is lacking in a motor speech area, as some authorities contend, there is a reasonable possibility, it appears to me, that under the proposed technique it might develop human speech in the same natural manner that it is integrated by the child. Witmer (**24**), Garner (**5**) and Furness (**4**) in fact, have all reported cases of articulation accurately imitative of human speech in trained anthropoid apes. The latter investigator not only succeeded in getting an orang-utan to utter the two words 'papa' and 'cup' but to say these words, so he believed, both intelligently and meaningfully. The progress in the speech training was, however, slow and laborious, but this again can be accounted for on theoretical grounds by the too-mature age of the animal at the time its education was undertaken.

7. A Valid Basis of Comparison

The present relative position of civilized man and the ape and the proposed plan for equating extraneous influences are schematically indicated in the accompanying diagram. Here M′ represents man as developed on the highest environmental level. M then indicates the original basic man evolved within the lower stratum of uncivilized surroundings. The point A on the parallellogram may serve for the ape who

grows up in a situation similar to that of the man, M. The resulting organisms, whether M or A, apparently differ but little, as we have tried to show throughout this paper. That comparisons which have heretofore been made, namely, those between civilized man, M′, and the ape, A, are spurious, however, should now appear from the diagonal M′A, since



two variables, one of which has previously gone unconsidered, are present in such a comparison. The first of these variables is the difference in organisms, and the second is the difference in training or environment. It remains, however, to complete the figure by means of the experiment outlined. The resulting organism, A′, reared upon the same environmental level with M′ then becomes comparable to civilized man with the same degree of validity that A and M are comparable at the present time.

8. Possibility of Negative Results

There can be little doubt that the conceptions upon which the foregoing remarks are based are fundamental. It would be presumptuous, however, to attempt any theoretical interpretation of proposed results before those results, whether they be of value or not, have been attained. It may not be amiss therefore at this point to note that the writer subscribes to no particular theory as a possible explanation of the differences which are supposed to exist between the capacities of man and the infrahumans. Although it may

appear from a perusal of these paragraphs that a defense for radical environmentalism has been propounded, such is by no means the object of this report. If undue emphasis seems in fact to have been placed upon environmental factors it is only because the project outlined is more favorably presented from such a position and because one of the chief desiderata has been to point out the importance of extraneous developmental influences.

I fully appreciate furthermore that in presenting arguments intended to emphasize what to me appears to be the importance of this proposed investigation I have subjected myself to possible criticism on the grounds of gross anthropomorphism. In order therefore to give adequate weight to the opposite side of the case we list below a few fairly obvious morphological and physiological distinctions which suggest possible differences in behavior between the ape and the child even though environmental factors are equated as completely as possible.

(*a*) The first and most significant of these is the considerably smaller brain size of the anthropoids.[7] Upon the assumption of a close correlation between neurological development and behavioral capacity, the ape might thus at the start be expected to be inferior to the human in tasks of any complexity.

(*b*) The relatively longer and stronger arms of the anthropoid, it may be supposed, would lead to greater aptitude in climbing than that possessed by the human. To the extent that it became necessary to curb this supposed tendency to climb more in one organism than in the other the upbringing of the two would necessarily differ.

(*c*) None of the anthropoid apes possesses the *opposable thumb*, with the power of bringing the thumb against each of the fingers, which is so well developed in man (**14**). Probably this would result in differences in manual dexterity. The greater facility with which the ape manipulates objects with its feet could be readily eliminated, however, by the use of shoes.

[7] F. Tilney (**17**, II, p. 567) reports that the chimpanzee brain occupies about one-fourth the volume of the human brain.

(*d*) If the orang-utan were selected for an experiment of this kind, a further structural distinction is to be pointed out, since this ape when walking erect steps on the outside of its feet, curling the toes inward. The chimpanzee and gorilla, however, tread on the soles of the feet like man.

(*e*) The somewhat more rapid rate at which the anthropoid infant begins to crawl and stand is also to be remarked. It has been maintained that an ape of one year is about equivalent in physiology and behavior to the human baby of twice that age. The period of adolescence and the growth of the permanent teeth, however, suggest that the ape during later childhood is in advance of the human by an interval variously estimated at from two to four years. Giving the human baby in this experiment a year's advantage in age would do much to balance this inequality.

(*f*) Other less apparent distinctions would doubtless appear when direct comparisons could be made.

It is entirely possible, therefore, that the findings of the proposed investigation would be predominantly negative in character. I cannot believe, however, that this would seriously vitiate its significance, since either negative or positive results should be of some importance not only to psychology and education, but to biology and sociology as well. Some light in addition should be thrown by this means upon the ancient controversy between the environmentalists and the hereditarians. If demonstrable differences in behavior existed at any given stage of training, and if the environmental factors had without question been held constant throughout that training, then the conclusion that the differences were due to native influences would be well-nigh unimpeachable. It could be maintained, should such results be secured, that the ape, given full opportunities to acquire a complete repertoire of human reactions, had progressed only part of the way.

9. Summary

The chief points which we have endeavored to bring out in the preceding sections may be summarized briefly as follows:

1. There is some evidence which indicates that human children, if kept throughout the early impressionable years in surroundings similar to those of wild animals, develop permanent behavior traits which are more like those of animals than of humans.

2. Comparative psychology, however, seems largely to have overlooked the tremendous rôle played by environmental influences upon captive wild animals before they are captured and brought to the laboratory. It is probable in fact that anthropoid apes have rarely if ever been obtained for experimental purposes at young enough ages to preclude their already having acquired basic infrahuman modes of reaction.

3. A further criticism which may be levied against most experiments with the higher primates is that the experimenter and all others who observe captive specimens, although meticulous with reference to the elimination of 'secondary cues' during laboratory tests, are likely to introduce unwittingly *primary cues* of a seriously disturbing nature when experiments are not in progress. The effect of this extra-experimental stimulation may be not only to stamp-in existing animal reactions, but even to integrate in the animals many additional responses of the same character. The current practices of confining anthropoids in cages most of the time and of leading them about on chains, must certainly be conducive as well to typical infrahuman activity.

4. We have suggested that procedures of this sort are at least partly responsible for the failure to elicit more human-like behavior from anthropoid apes. Such factors may therefore be regarded, it appears to us, as invalidating in large measure those conclusions which infer that the ape is *naturally* inferior in various capacities to man.

5. Since it is manifestly impossible to impress upon a human subject environmental influences identical to those of captive animals, we have proposed as a fair experiment which will permit a valid comparison of the behavior of these organisms that an infant ape be adopted *at birth* into a human family and be raised, not as a pet, but in all respects exactly

as a child. It has been suggested furthermore that the ideal situation would be to bring up the anthropoid with a human baby of about the same age, so that genetic case studies of the two individuals would be possible.

6. How far the ape would develop in these surroundings is of course a matter of conjecture, but the possibility cannot be denied that if this animal is at all capable of acquiring human speech, it would probably do so in a situation of this kind.

7. We have also tried to consider at some length the opposite possibility, *viz.*, that the outcome of this genetic investigation would be chiefly negative so far as the animal subject is concerned. In either event it has seemed to us that the results attained would adequately compensate for the difficulties to be encountered in such an undertaking, since the findings of an experiment of this nature, regardless of its outcome, should be of considerable scientific importance.

Arrangements for the carrying out of an experiment of the type outlined with an anthropoid ape and a human child are at present being formulated. If the plans can be satisfactorily consummated we hope to be able through later papers to report the progress of the work.

10. Bibliography

1. Alpert, A., The solving of problem-situations by preschool children; an analysis, *T. C. Contrib. to Educ.*, 1928, No. 323, 69 p.
2. Conradi, E., Song and call-notes of English sparrows when reared by canaries, *Amer. J. Psychol.*, 1905, **16**, 190–98.
3. Cunningham, A., A gorilla's life in civilization, *Bull. Zoöl. Soc. of N. Y.*, 1921, **24**, 118–24.
4. Furness, W. H., Observations upon the mentality of chimpanzees and orang-utans, *Proc. Amer. Phil. Soc.*, Phila., 1916, **55**, 283–85.
5. Garner, R. L., Gorillas and chimpanzees, London, 1896.
6. Gesell, A., Infancy and human growth, New York, Macmillan, 1928, 418 p.
7. Hicks, V. C. and Carr, H. A., Human reactions in a maze, *J. Anim. Behav.*, 1912, **2**, 98–125.
8. Hobhouse, L. T., Mind in evolution, New York, Macmillan, 1901, 415 p.
9. Koffka, K., The growth of the mind, New York, Harcourt, Brace, 1925, 382 p.
10. Köhler, W., The mentality of apes, New York, Harcourt, Brace, 1925, 342 p.
11. Kohts, N., Untersuchungen über die Erkenntnisfähigkeiten des Schimpansen, Moscow, 1924. (Summarized by R. M. Yerkes and A. Petrunkevitch, *J. Comp. Psychol.*, 1925, **5**, 98–108.)

12. MONRO-WARK, P., Mrs. McCoy coaches feathered prima donnas, *American Magazine*, March 1930, **109**, 69–70.
13. MURPHY, G., Historical introduction to modern psychology, New York, Harcourt, Brace, 1929, 470 p.
14. OSBORN, H. F., Men of the old stone age, New York, Scribners, 1915, 545 p.
15. SCOTT, W. E. D., Data on song in birds, *Science*, 1901, **14**, 522–26 and 1902, **15**, 178–81.
16. SQUIRES, P. C., 'Wolf-children' of India, *Amer. J. Psychol.*, 1927, **38**, 313–15.
17. TILNEY, F., The brain from ape to man, New York, Hoeber, 1928, 2 vols.
18. TINKLEPAUGH, O. L., An experimental study of representative factors in monkeys, *J. Comp. Psychol.*, 1928, **8**, 197–236.
19. TREDGOLD, A. F., Mental deficiency, New York, William Wood, 1915, 491 p.
20. WARDEN, C. J., The ability of 'Fellow,' famous German shepherd dog, to understand language, *J. Genet. Psychol.*, 1928, **35**, 330–31.
21. WARDEN, C. J., The sensory capacities and intelligence of dogs with a report on the ability of the noted dog 'Fellow' to respond to verbal stimuli (Proc. Galton Soc.), *Eug. News*, 1928, **13**, 2–6.
22. WARDEN, C. J. AND BAAR, J., The Müller-Lyer illusion in the ring dove, *Turtur risorius*, *J. Comp. Psychol.*, 1929, **9**, 275–92.
23. WARDEN, C. J. AND WARNER, L. H., The sensory capacities and intelligence of dogs, with a report on the ability of the noted dog 'Fellow' to respond to verbal stimuli, *Quar. Rev. Biol.*, 1928, **3**, 1–28.
24. WITMER, L., A monkey with a mind, *Psychol. Clin.*, Phila., **3**, 179–205.
25. YERKES, R. M., Almost human, New York, Century, 1926, 278 p.
26. YERKES, R. M., The great apes, New Haven, Yale Univ. Press, 1929, 652 p.
27. YERKES, R. M., The mental life of monkeys and apes; a study of ideational behavior, *Behav. Monog.*, 1916, **3**, No. 1, 145 p.
28. YERKES, R. M., The mind of a gorilla, *Genet. Psychol. Monog.*, 1927, **2**, Nos. 1–2, 1–193.
29. YERKES, R. M., The mind of a gorilla: Part II. Mental development, *Genet. Psychol. Monog.*, 1927, **2**, 375–551.
30. YERKES, R. M., The mind of a gorilla: Part III. Memory, *Comp. Psychol. Monog.*, 1928, **5**, No. 2, 92 p.
31. YERKES, R. M., A program of anthropoid research, *Amer. J. Psychol.*, 1927, **39** (Washburn Com. Vol.), 181–99.
32. YERKES, R. M. AND LEARNED, B. W., Chimpanzee intelligence and its vocal expressions, Baltimore, Williams & Wilkins, 1925, 157 p.

[MS. received October 22, 1930]

DISCUSSION

THE PSYCHOLOGIST'S CIRCLE

The dilemma between behaviorism, on the one hand, and introspectionism or Gestalt psychology, on the other, is not, it seems to me, a true dilemma at all but a circle, of which one party sees principally the one side, and the other party the other. While it is nothing new to point out the existence of this circle, there is nevertheless a need to insist upon it as long as psychologists argue over epistemological matters. If there is a way of escaping from the circle—and it seems to me that there is,—then the whole matter becomes very important. I wish therefore to describe the circle and to point a way out of it.

The Circle

The circle (Fig. 1) has orientation in respect of two poles.

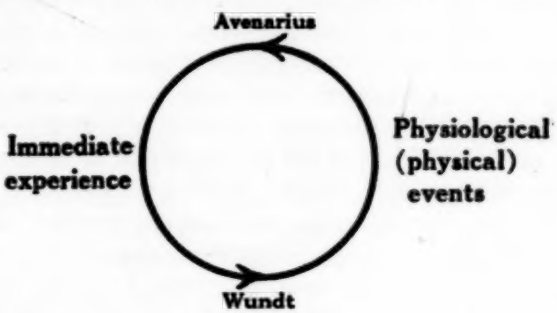


Fig. 1.

On the one hand, there is experience from which all scientific facts are inferentially derived. The initial position of experience in the scientific process can not be denied. It is the starting point of all empirical, and therefore experimental, inference. Wundt called it 'immediate experience' and placed the conscious actualities there.[1] Mach called it 'sensation' and based physics as well as psychology upon it.[2] Köhler has recently used the term 'direct experience' for it.[3] Phenomenology, as a *Vorwissenschaft*, is said

[1] See, *e.g.*, W. Wundt, Grundriss der Psychologie, 1896, sect. 1. *Cf.* note 7, *infra*.
[2] E. Mach, Analyse der Empfindungen, 1885, et seq., esp. chap. 1.
[3] W. Köhler, Gestalt Psychology, 1929, esp. chaps. 1–3.

to deal with it, but in general it is supposed to contain a large part of the subject-matter of introspective and Gestalt psychology.

On the other hand, there is the physical world of supposedly permanent objects, and this world includes for our special interest the physiological facts which the psychologist never escapes and which the behaviorist wishes not to escape. It is the other pole in the dichotomy of mind and body.

Because experimental psychology is inevitably a physiological psychology the relationship between the two poles becomes crucial, and the circle appears because the relationship reveals two opposing dependencies.

The one relationship makes psychology prior to physics. This view derives originally from Berkeley and the British empiricists. Mind generates matter; mind is given and we have only the problem of how belief in matter or external world ever came about. Thus Wundt called the conscious actualities 'immediate experience,' and argued that the facts of physics are derived inferentially from them and are 'mediate.' Mach was more concerned with the similarity between physics and psychology than with the difference, but it would seem, if physics is to be got from 'sensations,' that physics must be somehow mediate. Many psychologists have since emphasized the mediate, inferential nature of the physical sciences, and Köhler, in good Wundtial fashion, has recently sought to confound Watson by pointing out that the behaviorist is as dependent upon 'direct experience' as the physicist or the psychologist.[4]

This view that physical or physiological entities are derived from immediate experience expresses the belief in a generative dependence of the physical upon the psychical, and may be expressed by the arrow at the bottom of Fig. 1 and labelled 'Wundt.'

The other relationship is the converse. It finds that the phenomenal processes of introspective psychology must be regarded as dependent upon the nervous system. Psychophysical parallelism and epiphenomenalism suggest this view. Avenarius made it explicit when he defined the subject-matter of psychology as 'vital series' dependent upon the 'System C,' and, conversely, the subject-matter of physics as independent vital series.[5] Külpe in 1893 [6] and Titchener throughout his professional life [7] accepted

[4] Köhler, *op. cit.*, chap. 1.

[5] R. Avenarius, Kritik der reinen Erfarhung, 1888–90. *Cf.* note 7, *infra.*

[6] O. Külpe, Grundriss der Psychologie, 1893, 2.

[7] See E. B. Titchener, Text-book of Psychology, 1910, sects. 1, 2, 5, 6; Systematic Psychology: Prolegomena, 1929. Most of the latter book is relevant to the subject

Avenarius' view. "The subject-matter of psychology," said Titchener, "is the sum-total of human experience considered as dependent upon the experiencing person." [8] Titchener was rejecting Wundt's view here, because he believed that a scientific psychology, modeled on physics as the typical science, could not claim a logical priority to physics. The two sciences, he thought, are coördinate in the scheme of knowledge; they have the same subject-matter, experience, but they are differentiated by regarding it from the points of view of 'dependence' and 'independence' of the experiencing individual or the nervous system or Avenarius' System C.

While such points of view may be coördinate, the relationship of dependence has come to be thought of as a fact, and it is an asymmetrical relation. The dependence is not logically generative, as in the other case, but it 'exists.' We may represent this kind of dependence by the arrow at the top of Fig. 1, and label it 'Avenarius.'

There results the circle.[9] A physiological fact, as a special kind of physical fact, is derived from experience, but experience is itself dependent upon physiological processes. Sometimes we see one half of the circle, sometimes the other, but seldom both at once. Before a behaviorist can rule out consciousness as an epiphenomenon, he must have agreed with Avenarius to the extent of thinking that conscious processes are only to be understood in their dependence upon physiological processes. Köhler is quite right in saying then that the behaviorist would be nowhere if he really ignored the phenomena of direct experience, but Köhler leaves us

in hand, but see especially Titchener's summary, 259–266, his exposition and critique of Wundt, 98–113, and his favorable discussion of Avenarius, 113–119, 134–138. Titchener undertakes to show the separate status of physics, biology and psychology by a table that shows them in a hierarchy of dependencies, p. 266.

[8] Titchener, Text-book, 16.

[9] That the circle is not truly a circle is my reason for offering Fig. 2 for Fig. 1. My colleague, Professor C. I. Lewis, points out that experience, in what I have called the 'logically generative dependence' of brain on experience, is a *causa cognoscendi*, whereas the brain, in the existential dependence of experience on the brain, is a *causa essendi*. Fig. 2 has the advantage of letting experience appear always as *causa conoscendi* and the brain as a *causa essendi*. Lewis' book, Mind and the world-order, 1929, deals with this epistemological problem in a highly sophisticated manner of which many philosophers seem to have approved. My only excuse for touching so difficult an epistemological matter with a psychologist's naïvité is that psychologists will talk inexpertly about these things, and they may read a short simple note by me when they will not read a long involved book by a philosopher.

wondering where the direct experience, part of the subject-matter of psychology, comes from.

A circle is not necessarily an absurdity. It may be a necessary predicament. In this case, however, it would seem strange if psychology, with all its conscious effort to copy physics, had completely failed, had tried, like physics, to build up a structure mediate to experience, and had, after all its effort, only landed itself again in the lap of experience whence it set out. Must the circle stand?

The Way Out of the Circle

The solution of the problem lies, it seems to me, in a further analysis of the concepts which the circle involves. We must reconstruct the figure (Fig. 2).

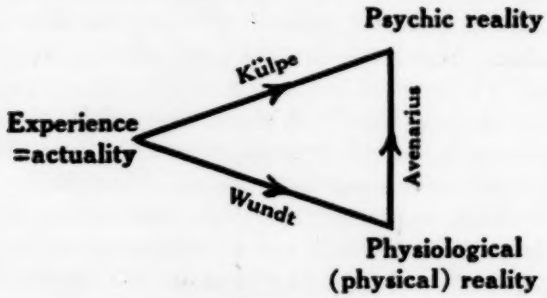


Fig. 2.

Of course we must start with experience. No other conception of the scientific process is feasible. And we can draw the line for Wundt from experience to the physiological and physical facts. An entity, like a brain process or an electron, is a construct. It is not immediately experienced, but it is generated out of many observations. In this sense it is logically mediate to experience itself. Here my personal preference is for the distinction between actuality and reality. Experience is actual. The electron or the process in the brain is real but not actual. Reality is always inferred from actuality.

Now it seems to me that Titchener was right in thinking that psychology and physics must be coördinate sciences, but that he was wrong in implying (as I think he did [10]) that the subject-matter

[10] This statement may be unfair to Titchener. I make it because I think that the belief that psychology is just as mediate to experience as physics leads at once to the conclusion that mental objects belong in psychology just as much as physical objects belong in physics, and I do not for a moment believe that Titchener would accept this conclusion.

of psychology is in any way more 'phenomenal,' 'immediate,' or 'direct' than the subject-matter of physics. A sensation or an emotion is just as much of a generalized reality, just as little an immediate actuality, as an electron or a neurone, and the law of association is just as much a result of an inferential process as the law of gravitation or the all-or-none law of nervous conduction. Külpe made the distinction between the 'conscious actual' and the 'psychic real',[11] and thus I have dared to place Külpe's name on the upper line of the triangle in Fig. 2. The figure now asserts that both psychology and physics are systems of reals derived by similar inferential, experimental processes from experience. I should not, however, like to call experience 'conscious', because the word 'conscious' implies to me a psychological reality.

What now has become of the dependence of which Avenarius spoke? Well, the dependence is a fact. Psychology has an asymmetrical relationship to physiology. We must put in the third side of the triangle for Avenarius and for everyone else who has noted this relationship. However, the relationship exists in reality, and does not have to come up behind experience to disestablish its priority.

This picture would not seem so strange if psychology had developed with more interest in the concept of the unconscious and less in psychophysics and psychophysiology, both of which emphasize the fact of dependence. The unconscious is not actual, but it is real, quite as real as an electron or energy. One can call all these things hypotheses if he wishes, but the line between an hypothetical entity and a reality is little more the threshold of subjective assurance.

The sense of the diagram in Fig. 2 can not become evident unless the scientist remembers that Experience cannot become the object of observation. For science it is simply a *Whence*, a premise to his method. Even the most direct observation is the beginning of inference, for it is never a mere phenomenological photography but always the working over of experience by an *Aufgabe* or a point of view.[12] If the psychologist, for instance, thinks that there is

[11] O. Külpe, Versuche uber Abstraktion, *Ber. ü. d. I Kongr. f. exper. Psychol.*, 1904, 56–68, esp. 66–68. Cf. C. Rahn, The relation of sensation to other categories of contemporary psychology, *Psychol. Monog.*, 1913, **16**, No. 67, 76–85. The distinction necessarily runs through Külpe's *Die Realisierung*, 1912–1923. Külpe's notion of conscious actuality (*Wirklichkeit*) must not be confused with Wundt's theory of actuality (*Aktualität*).

[12] I am thinking here of Yokoyama's experiment: see *Amer. J. Psychol.*, 1924, **35**, 301–304. An observer in a psychophysical experiment, who is trying to make the

nothing in the world but reality, then he had better correct Fig. 2 by rubbing out the left-hand corner, even though the metaphysician will put it right back in again.

EDWIN G. BORING

HARVARD UNIVERSITY

[MS. received November 18, 1930]

A NOTE ON DR. KLEIN'S PLEA FOR ECLECTICISM

In a recent paper in this journal, Dr. Klein has argued persuasively and at first view plausibly, the case of Eclecticism vs. System-Making in Psychology.[1] He decides that "judicious eclecticism in the choice and application of explanatory principles, even if this means an unfinished science seems to us to be preferable to a partisan attachment to a single principle, in the interests of a finished system." After careful consideration of his article, the present writer is inclined to believe that in developing the case for eclecticism Dr. Klein has himself fallen into the trap against which he warns us. He writes that "the primary evil of system-making is its tendency to either ignore or distort facts that fail to fit into the system." It is the writer's contention that in trying to carry his point Dr. Klein makes just this mistake. His argument for eclecticism is based largely on an analogy drawn from the physical sciences. The writer believes he can show Dr. Klein's two chief arguments relative to uniformity in the physical sciences to be based on ignored or distorted facts and hence invalid.

In the first place, Dr. Klein argues that a consideration of the methodology of physics shows us that such principles as that of Archimedes and such laws as that of Ohm are not made the key to the solution of all physical problems. This is perfectly true and

'immediate' judgment as to which of two rectangles, exposed for a small fraction of a second, is the longer, finds, under the introspective *Aufgabe*, that the judgment is not immediate after all. He must often decide whether the conscious residues of an already past experience are adequate surrogates of that experience, make a decision and report it, so that the inferential process, which is supposed to belong to the experimenter, is found to extend back into the act of observation itself. When the judgment is purely automatic, the observer does not think that he has made an observation. It is to him as if an unconscious observer had made and uttered the judgment, and he is left merely listening to the sound of his own voice.

[1] D. B. Klein, PSYCHOL. REV., 1930, 37, 488–496.

the present writer is willing to grant it without further ado. Dr. Klein, however, ignores completely that which makes physics the established science, par excellence. Such laws and principles as Dr. Klein mentions can be and are handled in terms of the mathematical theory of physics or what one might call the system of physics.[2] To take only the two examples we have already mentioned. Archimedes' principle is simply a special case of the general principle of equilibrium of rigid bodies.[3] Similarly, Ohm's law is derived from the general laws of the stationary electromagnetic field.[4] Both fit admirably into the 'systems' of theoretical physics. As a matter of fact, in the history of physics all observations and principles were the subject matter of argument and dispute almost as bitter as that which goes on in psychology to-day. Such dispute continued until they could be made to fit into these systems. Nor does one have to go back to the discussion of conservation of movement between Leibnitz and Descartes to exhibit this. A consideration of physical journals from the Michelson-Morley experiment in 1886 until the general acceptance of Einstein's special theory about 1912 should elucidate this fact.[5] One does not have to go back to the struggle in the eighteenth century between the corpuscular and the wave theory of light. It exists today. Also the very recent discussion regarding causality and probability in physics should not be overlooked.[6]

Turning to the psychological systems, Dr. Klein implies that, whereas the physicist does not make Archimedes' principle the key to all physical problems, the psychologist is often guilty of methodological errors of this sort. His chief target is the Gestalter. Let us see if Dr. Klein's implication is justified. Does the Gestalter attempt to explain after-images of the total visual field,[7] and the forgetting of an intended act [8] by Korte's laws? He certainly does

[2] Prof. Planck of the University of Berlin always calls his final course in theoretical physics 'System der Physik'. Prof. Schroedinger, his successor, gave up the title in 1930, as being a little philosophical.

[3] *Cf.* M. Planck, Einführung in die Theoretische Physik, Vol. 1, pp. 165–167. Berlin, 1928. After deriving Archimedes' Principle, Planck writes: "So entspringen aus dem allgemeinen Gleichgewichtsprinzip für starre Körper auch die in der Hydrostatik und Aerostatik grundlegenden Sätze."

[4] Planck, *op. cit.*, Vol. 3, pp. 105–107.

[5] There are still a few 'voices in the wilderness' crying against Einstein. *Cf.* C. L. Poor, What Einstein really did, *Scribner's*, November 1930.

[6] *Cf.* H. Dingler, Der Zusammenbruch der Wissenschaften etc., Munich, 1926.

[7] W. Jablonski, *Psychol. Forsch.* 1930, 13, 146–197.

[8] C. Birenbaum, *Psychol. Forsch.* 1930, 13, 218–285.

not. On the contrary he attempts to explain all three (and of course, many others) in terms of the laws of organized wholes. The 'system' is actually following the methodology of the physical sciences.

Dr. Klein's second chief point is to put the question: Why do we have 'Psychologies of 1925' and no other '—ologies of 1925'? His answer is, of course, that the other sciences are 'eclectic.' This is very far from the truth. Actually, the book 'Psychologies of 1925' is a cross-section of theoretical views on several different topics by such psychologists as are thinking theoretically along these lines. The catch phrase used as a title is more than a little misleading. There is absolutely no reason why there should not be a 'Biologies of 1925' by such men as T. H. Morgan, J. Schaxel, O. Hertwig, H. Speman, R. Harrison, E. Rignano, etc. As a matter of fact, in L. Bertalanffy's work, we have just such a survey, done, of course, by one man,[9] not several. This theorist of science pictures the biology of 1928 as having internal conflicts equally as serious as those that characterize the 'Psychologies of 1925'.

Turning even to physics; a book entitled Molecular Physics of 1930 with articles by such men as Born, Schroedinger, Bohr, Planck, de Broglie, etc. would not only lack absurdity but would be very useful.

That theoretical conflicts within psychology have a particularly belligerent aspect and that superficial analysis of the situation might easily lead one to believe with Dr. Klein that psychology falls into methodological pitfalls which her sister sciences avoid, is not here to be denied. Whatever differences do exist between psychological system-making and that of other sciences are of degree and *not* of kind. The writer believes two factors are chiefly the cause for these differences. The first (of lesser importance) is to be found in the type of personality that enters psychology. To tread on no toes, this will not be enlarged upon here.

The second (of greater importance) lies in the fact that psychology is a *very* young science. One must only remember that Fechner's 'Elements of Psychophysics' carries a lengthy section which attempts to disprove Descartes' assertion that the conarium is the seat of the soul. Recent researches into the theory of science would lead us to believe that the growth of any science is an organic function with certain well-marked developmental stages. It is doubtful if it can be yet said that psychology has moved from the

[9] L. v. Bertalanffy, Kritische Theorie der Formbildung, Berlin, 1928.

Aristotelian to the Galilean level of thought.[10] When the science of psychology does make this move more thanks will be due to system-making than to eclecticism. There were many eclectics among the physicists in 1847 and 1905. It is to the system-making of Helmholtz, however, that we owe the first law of thermodynamics. It is to the system-making of Einstein that we owe the theory of relativity.

In closing this note, the writer would like to state that he believes firmly in the worth of physical analogy for the treatment of psychological problems. To use such analogy to advantage, however, one must have a clear understanding of the actual way that the physicist goes about the experimental investigation of his problems and the building of his theories.

J. F. Brown

University of Colorado

[10] *Cf.* K. Lewin, The transition from Aristotelian to Galilean level of thought in psychology, *J. Gen. Psych.*, 1930 (in press).

[MS. received December 5, 1930]

PSYCHOLOGICAL REVIEW PUBLICATIONS

Original contributions and discussions intended for the Psychological Review should be addressed to

Professor Howard C. Warren, Editor PSYCHOLOGICAL REVIEW,
Princeton University, Princeton, N. J.

Original contributions and discussions intended for the Journal of Experimental Psychology should be addressed to

Professor Samuel W. Fernberger, Editor JOURNAL OF EXPERIMENTAL PSYCHOLOGY,
University of Pennsylvania, Philadelphia, Pa.

Contributions intended for the Psychological Monographs should be addressed to

Professor Raymond Dodge, Editor PSYCHOLOGICAL MONOGRAPHS,
Kent Hall, Yale University, New Haven, Conn.

Reviews of books and articles intended for the Psychological Bulletin, announcements and notes of current interest, and *books offered for review* should be sent to

Professor Edward S. Robinson, Editor PSYCHOLOGICAL BULLETIN,
Kent Hall, Yale University, New Haven, Conn.

Titles and reprints intended for the Psychological Index should be sent to

Professor Walter S. Hunter, Editor PSYCHOLOGICAL INDEX,
Clark University, Worcester, Mass.

All business communications should be addressed to

Psychological Review Company, Princeton, New Jersey

THE PSYCHOLOGICAL REVIEW

is indexed in the

International Index to Periodicals

to be found in most public and college libraries

DIRECTORY
OF
AMERICAN PSYCHOLOGICAL PERIODICALS

American Journal of Psychology—Ithaca, N. Y.; Cornell University.
Subscription $6.50. 624 pages annually. Edited by M. F. Washburn, K. M. Dallenbach, Madison Bentley, and E. G. Boring.
Quarterly. General and experimental psychology. Founded 1887.

Journal of Genetic Psychology—Worcester, Mass.; Clark University Press.
Subscription $7.00. 600 pp. annually. Edited by Carl Murchison.
Quarterly. Child behavior, animal behavior, comparative psychology. Founded 1891.

Psychological Review—Princeton, N. J.; Psychological Review Company.
Subscription $5.50. 540 pages annually. Edited by Howard C. Warren.
Bi-monthly. General psychology. Founded 1894.

Psychological Monographs—Princeton, N. J.; Psychological Review Company.
Subscription $6.00 per vol. 500 pages. Edited by Raymond Dodge.
Without fixed dates, each issue one or more researches. Founded 1895.

Psychological Index—Princeton, N. J.; Psychological Review Company.
Subscription $4.00. 300–400 pages. Edited by Walter S. Hunter.
An annual bibliography of psychological literature. Founded 1895.

Psychological Bulletin—Princeton, N. J.; Psychological Review Company.
Subscription $6.00. 720 pages annually. Edited by Edward S. Robinson.
Monthly (10 numbers). Psychological literature. Founded 1904.

Archives of Psychology—New York, N. Y.; Columbia University.
Subscription $6.00. 500 pages annually. Edited by R. S. Woodworth.
Without fixed dates, each number a single experimental study. Founded 1906.

Journal of Abnormal and Social Psychology—Eno Hall, Princeton, N. J.; American Psychological Association.
Subscription $5.00. 448 pages annually. Edited by Henry T. Moore.
Quarterly. Abnormal and social. Founded 1906.

Psychological Clinic—Philadelphia, Pa.; Psychological Clinic Press.
Subscription $3.00. 288 pages. Edited by Lightner Witmer.
Without fixed dates (9 numbers). Orthogenics, psychology, hygiene. Founded 1907.

Psychoanalytic Review—Washington, D. C.; 3617 10th St., N. W.
Subscription $6.00. 500 pages annually. Edited by W. A. White and S. E. Jelliffe.
Quarterly. Psychoanalysis. Founded 1913.

Journal of Experimental Psychology—Princeton, N. J.; Psychological Review Company.
Subscription $6.00. 500 pages annually. Edited by Samuel W. Fernberger.
Bi-monthly. Experimental psychology. Founded 1916.

Journal of Applied Psychology—Baltimore, Md.; Williams & Wilkins Company.
Subscription $5.00. 400 pages annually. Edited by James P. Porter.
Bi-monthly. Founded 1917.

Journal of Comparative Psychology—Baltimore, Md.; Williams & Wilkins Company.
Subscription $5.00 per volume of 450 pages. Three volumes every two years. Ed. by Knight Dunlap and Robert M. Yerkes. Founded 1921.

Comparative Psychology Monographs—Baltimore, Md.; The Johns Hopkins Press.
Subscription $5.00. 400 pages per volume. Knight Dunlap, Managing Editor.
Published without fixed dates, each number a single research. Founded 1922.

Genetic Psychology Monographs—Worcester, Mass.; Clark University Press.
Subscription $7.00 per volume of 500–600 pages. Two volumes per year. Edited by Carl Murchison. Monthly. Each number one complete research. Child behavior, animal behavior, and comparative psychology. Founded 1925.

Psychological Abstracts—Eno Hall, Princeton, N. J.; American Psychological Association.
Subscription $6.00. 700 pages annually. Edited by Walter S. Hunter.
Monthly. Abstracts of psychological literature. Founded 1927.

Journal of General Psychology—Worcester, Mass.; Clark University Press.
Subscription $7.00. 500–600 pages annually. Edited by Carl Murchison.
Quarterly. Experimental, theoretical, clinical, historical psychology. Founded 1927.

Journal of Social Psychology—Worcester, Mass.; Clark University Press.
Subscription $7.00. 500–600 pages ann. Ed. by John Dewey and Carl Murchison.
Quarterly. Political, racial, and differential psychology. Founded 1929.